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VOL. XXIV.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Dr. John Bostock ; James Burney, Esq. ; J. B. ; E. F. G. H. ; J. B. van Mons ; Mrs. Agnes Ibbetson ; W. Saint, Esq. ; Mr. B. Cook ; Mr. J. Acton ; Mr. R. B. Bate ; James Staveley, Esq. ; Sir George Cayley, Bart. ; Mr. G. J. Singer ; R. Z. A. ; W. N. ; M. le Comte de Bournon, F. R. and L. S. ; Mr. Robert Lyall ; Mr. P. Barlow ; J. F. ; Mr. Robert Bancks.

Of Foreign Works, Prof. F. R. Curaudau ; M. Gay-Lussac ; M. Thenard ; M. Alex. Brongniart ; Prof. Lenormand ; M. Hassenfratz ; M. Haüy ; M. Rampasse ; Prof. Picot ; M. L. Cordier ; M. Descotils ; P. A. Steinacher ; M. Bouillon-Lagrange ; M. Vogel ; M. Fourcroy ; M. Vauquelin ; M. Cuvier ; M. Chaptal ; M. Berthollet, Jun. ; M. Klaproth ; M. Bucholz ; M. Berthier.

And of British Memoirs abridged or extracted, Humphry Davy, Esq. Sec. R. S. F. R. S. Ed. and M. R. I. A. ; Capt. W. Bolton ; Capt. H. L. Ball ; Mr. John Tad ; Mr. W. Barlow ; J. G. Children, Esq. F. R. S. ; Wm. Henry, M. D. F. R. S. V. P. of the Lit. and Phil. Soc. and Physician to the Infirmary at Manchester ; James Rennel, Esq. F. R. S.

The Engravings consist of 1. Captain Bolton's improved Jury Mast ; 2. Captain H. L. Ball's Method of Fishing an Anchor ; 3. Captain Ball's improved Anchor ; 4. Mr. J. Tad's Method of causing a Door to open over a Carpet ; 5. Mr. W. Barlow's Wrench for Screw Nuts of any Size ; 7. The Sting of the Nettle, highly magnified, in its natural State, emitting its Poison, and when broken ; 8. The Awn of the Indian Grass, used in Captain Kater's Hygrometer ; 9. The Leaf and Stem of the Sensitive Plant, showing their Structure ; 10. The Spiral Wire and its Case greatly magnified ; 11. Luminous Meteors, seen during a Thunderstorm, by James Staveley, Esq. ; 12. Diagrams to illustrate the Theory of Aerial Navigation, by Sir George Cayley, Bart. ; 13. A Machine that will ascend into the Air of itself by mechanical Means ; 14. A Machine with which a Man may raise himself into the Air ; 15. Figures illustrating the Crystallization of Endellion, by the Count de Bournon ; 16. Diagrams for a Demonstration of the Cotesian Theorem, by Mr. P. Barlow ; 17. Various Delineations and Sections of Grafts and Buds, from original Drawings after Nature, by Mrs. Agnes Ibbetson ; 18. Branch of a Portugal Laurel, from which the Bark had been accidentally separated ; 19. Different Structures of several Kinds of Wood.

TABLE

TABLE OF CONTENTS

TO THIS TWENTY-FOURTH VOLUME.

SEPTEMBER, 1809.

Engravings of the following Subjects: 1. Captain Bolton's improved Jury Mast
2. Captain H. L. Ball's Method of Fishing an Anchor: 3. Captain Ball's improved Anchor: 4. Mr. J. Tad's Method of causing a Door to open over a Carpet: 5. Mr. W. Barlow's Wrench for Screw Nuts of any Size.

I. On the Union of Tan and Jelly: by John Bostock, M. D.	1
II. The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, &c. By Humphry Davy, Esq. Sec. R. S. F. R. S. Ed. and M. R. I. A.	12
III. Remarks on the Boracic Acid, addressed to the first Class of the Institute, December 19th, 1809, by F. R. Curaudau, Professor of Chemistry applicable to the Arts, and Member of several literary Societies.	24
IV. Abstract of a Paper on the Decomposition and Properties of Fluoric Acid, presented the 9th of January to the Mathematical Class of the Institute, by Messrs. Gay-Lussac and Thenard.	29
V. Description of a Process, by Means of which Potash and Soda may be metallized without the Assistance of Iron; read before the French Institute the 18th of April, 1808; by F. R. Curaudau.	37
VI. Observations and Experiments on the Nature of the New Properties of the Alkaline Metals; by the same	40
VII. Improved Method of Forming Jury Masts: by Captain William Bolton of the Royal Navy	44
VIII. An Improvement of the Construction of Anchors, to render them more durable and safe for Ships: with an improved Mode of Fishing Anchors. By Captain H. L. Ball, of the Royal Navy.	46
IX. Observations on the Progress of Bodies floating in a Stream: with an Account of some Experiments made in the River Thames, with a View to discover a Method for ascertaining the Direction of Currents. By James Burney, Esq.	49
X. New Method proposed for measuring a Ship's Rate of Sailing. By the same Gentleman.	57
XI. Method of preventing Doors from dragging on Carpets, or admitting Air underneath them. By Mr. John Tad	59
XII. Description of an improved Screw Wrench, to fit different sized Nuts, or Heads of Screws. By Mr. William Barlow.	61
XIII. On the Measurement of Heights by the Barometer. In a Letter from a Correspondent.	63
XIV. On the Glauberite. By Alexander Brongniart	65
XV. An excellent colourless Copal Varnish. By Mr. Lenormand, late Professor of Natural Philosophy.	67
Scientific News	68
Meteorological Table	80

CONTENTS.

v

OCTOBER, 1809.

Engravings of the following Subjects: 1. The Sting of the Nettle, highly magnified, in its natural State, emitting its Poison, and when broken: 2. The Awn of the Indian Grass, used in Captain Kater's Hygrometer: 3. The Leaf and Stem of the Sensitive Plant; showing their Structure: 4. The Spiral Wire and its Case greatly magnified.	
I. Farther Application of a Series to the Correction of the Height of the Barometer	81
II. On the Action of the Metal of Potash on Metallic Salts and Oxides, and on Alkaline and Earthy Salts. By Messrs. Thenard and Gay-Lussac.	92
III. The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, &c. By Humphry Davy, Esq. Sec. R. S. F. R. S.-Ed. and M. R. I. A.	95
IV. Extract of a Letter from Mr. J. B. van Mous, Member of the Institutes of France and Holland, to the Editor, on Atmospheric Phenomena	106
V. Remaining Proof of the Cause of Motion in Plants explained; and what is called the Sleep of Plants shown to be Relaxation only. By Mrs. Agnes Ibbetson.	114
VI. A curious Property of Single Repetends. In a Letter from W. Saint, Esq.	124
VII. On the Use of Iron for Stairs, and instead of the Timbers of Houses, as a Security against Fire. In a Letter from Mr. Benjamin Cook.	126
VIII. On Respiration. By Mr. J. Acton. In a Letter from the Author	130
IX. On the Camera Lucida. In a Letter from Mr. R. B. Bate	146
X. Account of some Experiments performed with a View to ascertain the most advantageous Method of constructing a Voltaic Apparatus, for the Purposes of Chemical Research. By John George Children, Esq. F. R. S.	150
XI. Report of a Memoir of Mr. Hassenfratz respecting the Alterations that the Light of the Sun undergoes in traversing the Atmosphere. By Mr. Haüy.	155
Scientific News	158
Meteorological Table	160

NOVEMBER,

NOVEMBER, 1809.

Engravings of the following Subjects: 1. Luminous Meteors, seen during a Thunder Storm, by James Staveley, Esq: 2. Diagrams to illustrate the Theory of Aerial Navigation, by Sir George Cayley, Bart.: 3. A Machine that will ascend into the Air of itself by Mechanical Means: 4. A Machine with which a Man may raise himself into the Air.

I. Account of some luminous Meteors seen during a Thunderstorm. In a Letter from James Staveley, Esq.	161
II. On Aerial Navigation. By Sir George Cayley, Bart.	164
III. On Electro-Chemical Experiments. By Mr. G. J. Singer	174
IV. Extract of a Letter from Mr. J. B. Van Mons to Mr. Sue, on different Subjects relating to Galvanism and Electricity.	179
V. Description of the Process employed to ascertain the Existence of Alumine in Meteoric Stones, by B. G. Sage, Member of the French Institute, Founder and Director of the First School of Mines	190
VI. Letter from Mr. Rampasse, formerly Officer in the Corsican Light Infantry, to Mr. Cuvier, on a Calcareous Breccia containing fossile Bones found in Corsica.	193
VII. Extract of a Letter from Professor Picot, of Geneva, to the Editors of the Bibliotheque Britannique, on Comets.	197
VIII. On the Influence the Shape of a Still has on the Quality of the Product of Distillation: by Mr. Curaudau, Member of the Pharmaceutical and several other Societies.	201
IX. On Vegetable Astringents. By John Bostock, M. D. Communicated by the Author.	204
X. Question on the Preparation of Cork for modelling. In a Letter from a Correspondent.	222
XI. On the Dusodile, a New Species of Mineral; by Mr. L. Cordier	223
XII. Memoir on the Triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. le Comte de Bournon, F. R. and L. S.	225
XIII. On Detonating Silver. By Mr. Descotils.	237
XIV. Process for making a Fine Lake.	238
XV. On the Blue Wolfsbane, by Philip Antony Steinacher.	238
Scientific News.	239
Meteorological Table.	240

DECEMBER.

CONTENTS.

vii

DECEMBER, 1809.

Engravings of the following Subjects, (In Two Quarto Plates :) 1. Figures illustrating the Crystallization of Endellion, by the Count de Bournon. 2. Diagrams for a Demonstration of the Cotesian Theorem; by Mr. P. Barlow.	
I. On Vegetable Astringents. By John Bostock, M.D. Communicated by the Author	241
II. Memoir on the triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. le Comte de Bournon, F. R. & L. S.	251
III. Of the Irritability of Vegetables. By Mr. Robert Lyall, Surgeon. Read at the Literary and Philosophical Society at Manchester, Oct. the 6th, 1809. Communicated by the Author.	261
IV. Demonstration of the Cotesian Theorem, by Mr. P. Barlow	278
V. On the Influence of Electricity on Flame: by Mr. Leopold Vacca, Colonel of the 23d Regiment of Light Infantry.	283
VI. Of the Action of Phosphorus and oxigenized muriatic acid Gas on Alkalis: by Messrs. Bouillon-Lagrange and Vogel.	285
VII. On the Chemical Analysis of the Onion. By Messrs Fourcroy and Vauquelin.	290
VIII. Abridgment of a Paper on the Species of Carnivorous Animals, the Bones of which are found mixed with those of Bears in Caverns in Germany and Hungary. By Mr. Cuvier.	295
IX. Account of some Colours for Painting, found at Pompeii; by Mr. Chaptal. Communicated to the First Class of the Institute, March the 6th, 1809.	302
X. Remarks on the Introduction of Air into the Blood through the Lungs, in Answer to Mr. Acton. In a Letter from a Correspondent.	307
XI. Letter from Mr. Robert Bangks concerning the Meteorological Journal.	308
Scientific News.	309
Meteorological Journal.	320

SUPPLEMENT.

SUPPLEMENT TO VOL. XXIV.

- Engravings of the following Objects: 1. Various Delineations and Sections of Grafts and Buds, from original Drawings after Nature, by Mrs. Agnes Ibbetson. 2. Branch of a Portugal Laurel, from which the Bark had been accidentally separated. 3. Different Structures of several Kinds of Wood.
- I. Memoir on the Triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. le Comte de Bournon, F. R. and L. S. - 321
- II. On the Effects produced by the grafting and budding of Trees. In a Letter from Mrs. Agnes Ibbetson. - - - 337
- III. On the Defects of grafting and budding. By Mrs. Agnes Ibbetson. 346
- IV. Experiments on Ammonia, and an Account of a new Method of analyzing it, by Combustion with Oxigen, and other Gasses; in a Letter to Humphry Davy, Esq. Sec. R. S., &c., from William Henry, M. D. F. R. S., V. P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester. 358
- V. Observations on the Composition of Ammonia. By Mr. Berthollet, jun. 374
- VI. Analysis of the Chinese Rice-Stone, with some Observations on the Yu. By Mr. Klaproth. - - - 375
- VII. On the Effect of westerly Winds in raising the Level of the British Channel. In a Letter to the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S. By James Rennel, Esq. F. R. S. - - - 379
- VIII. On Dead Lime. By Mr. Bucholz. - - - 381
- IX. On the Murates of Barytes and of Silver. By Berthier, Mine Engineer. 383

ERRATUM.

Page.	line.
278	14 for <i>Théoric</i> read <i>Calcul</i> .

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AND

THE ARTS.

SEPTEMBER, 1809.

ARTICLE I.

On the Union of Tan and Jelly: by JOHN BOSTOCK, M. D.

To Mr. NICHOLSON.

SIR,

DURING the course of the last spring I was engaged in a set of experiments, which may be considered as a continuation of those formerly made on the analysis of animal fluids*. My object was to enable the operator to apply the tests, which indicate the existence of the principal constituents of these fluids, albumen, jelly, and mucus, so as not only to discover the *qualities* of the compound, but the *quantities* of its ingredients. The results of my experiments have been, upon the whole, unsuccessful; and I have at present chiefly to announce the failure of the different expedients, which I employed to attain my object. It may not, however, be altogether useless, to lay my experience

Purpose of the author's inquiry.

* See Journal, vol. IX, p. 244.

before your readers; not merely because I have it in my power to state some few facts, that may be considered as an addition to our stock of knowledge, but still more, because I may induce some one more skilful than myself, to point out a method of accomplishing what I have hitherto attempted without success.

Jelly.

The substance upon which I first operated, and to which I shall principally confine my attention in the present paper, is jelly; the characteristics of which are its solubility

Its characters.

in water, its forming an insoluble compound with tan, and the property which its aqueous solution possesses of concreting by cold, and being redissolved by the application of heat. The problem which I was anxious to solve was,

Inquiry whether the precipitate by tan be proportionate to the quantity present.

whether the compound of tan and jelly be uniform, so that by saturating the gelatinous part of a solution with tan, and collecting the precipitate, we may, from its weight, (the quantity of tan employed being known) ascertain the amount of the jelly previously contained in the fluid. From the experiments that had been performed on the subject,

Mr. Biggin's attempt to ascertain the proportion of tan.

particularly those of Mr. Biggin and Mr. Davy, I conceived, that this would be found to be the case. The object of Mr. Biggin's experiments was to ascertain the proportion of tan in different barks, for which purpose he formed similar infusions of them, and precipitated the tan from each by a solution of glue. He employed the solution of glue always of the same strength, and by collecting the precipitates, he judged of the quantity of tan that had united itself to the glue, and thus of the proportion of it in the bark*. The experiments are important, as comparing the different barks with each other, and thus ascertaining their respective value as substances to be employed in the manufacture of leather; but it is obvious, that, unless the compound of tan and glue be uniform, they do not show the absolute quantity of tan in any given weight of bark. Mr. Davy, in his experiments on astringent substances, has pointed out, with his accustomed sagacity, the different effects that are produced in the union of solutions of tan and jelly, according to their degree of concentration; and has proved, that in

Mr. Davy shows the precipitate is proportionate to the strength of the solution.

* Phil. Trans. 1799, p. 260.

proportion to the strength of the solution, either of jelly or of tan, will be the weight of the precipitate obtained*. It would appear, that, when the solutions are much diluted, the attraction of both the jelly and the tan for the water, to a certain extent, counteracts their attraction to each other, and thus prevents a portion of them from being removed from the fluid. Mr. Davy, however, as well as Mr. Biggin, evidently seems to have conceived, that the substance which was precipitated in all instances possessed the same properties, and consisted of a uniform compound of the two ingredients. This opinion is the very foundation of the method which he employed in his analyses, and is directly asserted in different parts of his papers†.

Both suppose the precipitate to be a uniform compound.

With this impression it was, that I entered upon a set of experiments, which may be considered as the converse of those of Mr. Biggin and Mr. Davy. The object of these chemists was, by the agency of jelly, to remove all the tan from a vegetable infusion, and to estimate its quantity from the weight of the precipitate; while mine was, by means of tan, to ascertain the quantity of jelly that was contained in any animal fluid. In pursuing this investigation, the first point was to determine upon the most proper substance to employ as the reagent; for as it is difficult, if not absolutely impossible, to procure tan in a state of perfect purity, it became necessary to discover some vegetable infusion, which should always possess similar properties, and in which the quantity of tan should be known, without having recourse to any long calculation. My attention was naturally, in the first instance, directed to galls; and I expected, that by employing equal weights, infusing them in equal quantities of water, and for an equal length of time, fluids would have been formed always containing equal quantities of tan. But upon making repeated trials, I find that this is not the case; and it would appear from all the experience I have had upon the subject, that two parcels of galls will scarcely ever be procured, which will precisely agree in their

The author's proceeding the converse of theirs.

A uniform reagent requisite.

Galls.

Not uniform in their nature,

* Phil. Trans. 1803.

† Phil. Trans. 1803. Nicholson's Journal, vol. V, p. 259, 269, & alibi.

nature. If finely powdered galls be infused for two hours in 8 times their weight of boiling water, an infusion is formed, which is generally transparent, of a deep brown colour, and which contains about one tenth of its weight of solid matter. But although this is the usual result of the process, it is by no means constantly so. Frequently the infusion will be thick and muddy, will not be rendered clear by being passed through the filter, nor will it become so after standing at rest for several days; its colour also varies considerably, the brown tinge existing in different shades of intensity, and occasionally being exchanged for a bottle green. The quantity of solid matter contained in the fluid is seldom precisely the same in any two trials; although it is generally about one tenth, yet I have occasionally found it no more than one fourteenth. Although it may appear at first view somewhat singular, that such different effects should be produced by the same substance; yet, when we attend to the visible difference, that exists in gall nuts, we shall easily conceive how these variations may take place. The structure of galls appears to have been little attended to, and they have generally been spoken of as homogeneous bodies, before the accurate description of their several parts, that is given by the Mr. Aikins in their late valuable publication*.

or homogeneous in their structure.

An extract of them does not answer.

As it appeared impossible to employ a recent infusion of galls for the standard fluid, I thought of evaporating the infusion, and making use of a solution of the dried residuum. But I found, that this residuum, although formed from a perfectly transparent infusion, is not capable of being completely redissolved, owing to some change that has been effected on one or more of its constituents, probably the extract, by which it becomes no longer soluble in water. This circumstance forms an insuperable objection to the employment of the dried residuum as a standard, because the quantity of matter, depending upon the variable proportion of the soluble and insoluble part, or of the tan and extract, will scarcely ever be found the same in any two specimens upon which we may operate.

* Aikins' Chem. Dict. Art. Gall nut.

The infusion of galls, however prepared, seemed inadequate to the purpose of affording an accurate test for jelly, I thought therefore of employing the artificial tan discovered by Mr. Hatchett, because, being a substance formed by a specific chemical action, it may be supposed always to possess the same chemical properties. It was accordingly prepared by digesting powdered charcoal in nitric acid, and the result coincided entirely with the description of Mr. Hatchett; it was readily dissolved both in water and alcohol, it precipitated jelly from its solution, and also the nitromuriate of gold, the muriate of tin, the superacetate of lead, and the oxisulphate of iron. All these properties show its strong resemblance to the infusions obtained from astringent vegetables. I was however disappointed in not finding it to answer the purpose that I had in view. Although the artificial tan very readily afforded a precipitate from a gelatinous solution, yet the jelly seemed to be only imperfectly thrown down, the fluid remained muddy after the operation, and the precipitated matter could not be completely separated from it. This circumstance I found to take place with different portions of the artificial tan, which were each of them prepared with every attention to Mr. Hatchett's directions; and, I conceive, depends upon a quantity of undecomposed acid, which remains attached to the tan, and which cannot be entirely removed from it. This excess of acid was always found in my experiments, and must probably have existed in Mr. Hatchett's preparations, for he points out their property of reddening litmus as one that is characteristic of them*. To whatever cause we may ascribe it, it seemed to be a sufficient objection to the use of this substance as a test for jelly.

Mr. Hatchett's
artificial tan

formed an im-
perfect precipi-
tate,

owing to the
presence of un-
decomposed
acid.

Catechu was next tried, but without any better success. Independent of the difference which exists between different specimens of this substance, which is considerably greater than what is found in the infusion of galls, I have never met with any catechu which is entirely soluble in water. In the different trials that I made to procure standard

Catechu does
not answer.

* Phil. Trans. for 1805, p. 215.

Spontaneous
decomposition
of its infusions.

solutions of catechu, a portion appeared to be only suspended in the fluid, so that it remained muddy, and neither became transparent by standing, nor was the insoluble part removed by passing through a filter. The infusions of catechu likewise became altered by exposure to the atmosphere more rapidly than those of galls, a considerable portion of the substance that had been dissolved being gradually deposited. This deposition goes on so rapidly, as to exhibit an appearance something similar to the saline vegetation of certain salts, the catechu creeping up along the sides of the glass to some distance above the surface of the fluid. I was not able to detect any difference between the part of the catechu which is retained in solution, and that which is deposited, except that the last was of a lighter colour, and was less soluble in water; they both produced precipitates with jelly and the muriate of tin. The precipitate which the catechu forms with jelly, like that produced by the artificial tan, does not in general form a compact or solid mass, but makes the fluid turbid without entirely subsiding from it, nor is it rendered transparent by being passed through a filter. This circumstance, as well as its imperfect solubility, renders catechu inapplicable as a test for jelly.

Its precipitate
like that of ar-
tificial tan.

Extract of rha-
tany.

The next substance that I tried was the extract of rhatany, a preparation said to be brought from the Portuguese settlements in South America; and, in consequence of its tonic quality, lately proposed as an addition to the *materia medica*. It contains a large proportion of tan; from the experiments that I have made upon it, larger than any other astringent extract with which we are acquainted; it appears to be more homogeneous in its consistence, it is completely soluble in water, and seems to remove jelly from its solution more readily than the other substances which I had tried. These properties pointed it out as the most nearly approaching to what I was in search of.

Preparation of
the jelly.

Before giving an account of the result of the union of tan and jelly, it will be necessary to make some remarks upon the preparations of this latter substance. It has been stated upon the highest authority, that of Mr. Hatchett and Mr.

Davy,

Davy*, that isinglass consists nearly of pure jelly. I am not disposed to question the general fact; but I may mention, as the result of my own experience, that this is not always the case; and that, even in a majority of instances, the isinglass that is procured from the shops will be found to contain a considerable proportion of insoluble matter, which I conceive to be of the nature of coagulated albumen. The proportion of the matter soluble in water, which I regard as pure jelly, and of the insoluble part, is very various. In one instance, where the isinglass was boiled with twenty times its weight of water, the jelly that was formed, instead of holding in solution 5 per cent of solid matter, was found to contain not more than 3·8, and although the addition of more water carried off some of the substance which had been left at the first boiling, still more than $\frac{1}{10}$ of the isinglass was left, apparently incapable of farther solution. This difficulty is obviated by boiling isinglass in water, pouring off the jelly, and evaporating it to dryness; by which means a substance is procured, that is always ready for experiments. But as this operation is attended with some trouble, I wish to substitute for it a solution of glue, according to the process employed by Mr. Biggin. Glue is entirely soluble in water, and therefore does not present the objection that attaches to isinglass, yet there are some circumstances, which seem to render glue less eligible for the purpose of experiments. From the mode in which glue is prepared it might be supposed, that it would contain a quantity of albuminous matter; and I was confirmed in this opinion by finding, that a solution of it has a precipitate formed in it by being boiled with the oximuriate of mercury†. The quantity of muriate of soda that exists in glue must be considered as an impurity, which may have some effect upon the combination of jelly and tan. A more important circumstance, however, and one which appears to have been disregarded by those who have employed glue as a test for tan is, that, as it is usually prepared, it contains a considerable proportion of

Isinglass variable in the proportion of its gelatine,

but this may be separated.

Glue contains albumen,

muriate of soda,

water.

* Hatchett, Phil. Trans. 1800; Davy, Phil. Trans. 1803.

† This circumstance had been noticed by Dr. Thomson, Chem. Vol. V, p. 479.

and much
water.

Glue differs
considerably
from isinglass.

water. By permitting glue divided into small pieces to remain in a heat of about 150° for 24 hours, I found that it had lost $10\frac{1}{2}$ per cent of its weight. And even although we might have the glue in a state of complete dryness and purity, I should doubt whether it be a proper substance to employ on the present occasion. Although it possesses the properties which characterize jelly, yet a solution of glue will be found to differ from a solution of isinglass, while they both contain the same proportion of solid matter. This difference is the most remarkable with respect to their power of concretion. A solution of glue, which I found by evaporation to contain $\frac{1}{25}$ of its weight of solid matter, although strongly adhesive, remained quite fluid when cold, whereas a similar solution of isinglass jelly would have been perfectly concrete. Glue also differs from isinglass in being considerably more soluble in cold water. Glue broken into small pieces, and digested in ten times its weight of water, at the temperature of the atmosphere, was in 48 hours entirely broken down, and so far dissolved, that the upper part of the fluid strongly precipitated the infusion of galls. Pieces of isinglass treated in the same manner were softened, and had their bulk increased, but the fluid was scarcely affected by tan. These circumstances led me to regard glue as different from isinglass jelly, and as possessing in an inferior degree the characteristic properties of jelly.

Experiments
made with the
soluble parts
of isinglass and
extract of
rhatany.

From these different circumstances I determined to employ the soluble parts of isinglass, and the extract of rhatany, in my future experiments on the combination of tan and jelly. But before I enter upon a description of the results, that were obtained by the union of these substances, I think it necessary to point out the difficulty, which occurs in the prosecution of these experiments, particularly in the collecting of the precipitate. When the tan and jelly are not employed in a state of considerable concentration, and when they are not added together in that proportion, which seems to form the most perfect compound, the precipitate separates slowly from the fluid, or sometimes remains permanently suspended; and when it is passed through a filter, it adheres to the paper so strongly, that it cannot be completely

Difficulty of
collecting the
precipitate.

pletely removed from it. Nor could I obviate this objection by weighing the paper before and after the fluid had passed through it, and thus calculating the weight of the precipitate. I found that in this case the paper acquired weight, not only from the precipitated matter, but likewise from what was still retained in solution. When an infusion of rhatany in the proportion of 1 to 10 was passed through a paper filter, the filter when dried was found to have acquired an addition of not less than $\frac{1}{10}$ of its former weight. A solution of jelly of the same strength passed with difficulty through the paper, and a large part was detained by it. Hence it follows, that, except in those cases where the fluids neutralize each other, so as to precipitate all their contents, we cannot ascertain the amount of the precipitate from the weight gained by the filter. What has been said will be sufficient to show, that perfect accuracy cannot be attained in these processes, even were the compound of tan and jelly in all cases a uniform substance.

I was soon however convinced, that the substance formed by the union of tan and jelly varies considerably according to the circumstances under which it is formed, particularly according to the proportion in which the two ingredients are presented to each other. Without entering into a detail of the numerous trials, that I made upon this subject, I shall think it sufficient to give an account of one experiment, that may serve as a specimen of the rest. I must here remark, that, although my experiments agreed sufficiently to satisfy me respecting the nature of the conclusions that were to be deduced from them, yet I never performed two, in which the results exactly coincided.

The precipitate not uniform.

Three equal portions of the extract of rhatany were dissolved in ten times their weight of water; and three portions of jelly from isinglass were procured, bearing respectively the proportions of 8, 4, and 2 to the three portions of rhatany. These were also dissolved in equal quantities of water, kept soluble by heat, and added to the three portions of rhatany. Copious precipitates were produced in all of them, and after standing for some time, the supernatant fluids became clear. The precipitates were collected and dried by exposure to the same degree of heat. All the residual

Experiment.

dual fluids precipitated jelly, proving that they contained a quantity of uncombined tan ; but the precipitation was of course much less copious in the one which had received the smallest quantity of jelly. The weights of the precipitates were to each other in the ratio of 16, 9·5, and 7. As in all the cases the whole of the jelly had entered into combination, the proportion of the jelly to the tan might be estimated. In the first experiment, i. e. where 8 parts of jelly and 10 of tan were employed, the jelly and tan in the compound were nearly equal ; where 4 parts of jelly had been added to 10 of the rhatany, the proportion in the compound was as 42 to 58 ; and where only 2 parts of jelly had been employed, the compound consisted of 28·5 parts of jelly to 71·5 of rhatany. From these experiments we learn, that in proportion as the tan exists in excess more of it becomes united to the jelly ; so that if we were to attempt to estimate the amount of the jelly in any fluid by the weight of the compound which it forms with tan, we should much overrate the quantity of the jelly. Having found, that, where the solutions are employed in a state of considerable concentration, a compound is formed consisting of nearly equal weights of the two ingredients, we might conclude, that the quantity of jelly in the third experiment was in the proportion of 3·5, while in fact it was no more than as 2.

When the tan is in excess, more unites with the jelly.

Differences of the precipitates.

The physical properties of the precipitates were considerably different, so as to indicate a difference in their chemical composition. The first precipitate, which was composed of nearly equal parts of the two ingredients, was of a dark red colour, of a hard and brittle consistence, and presented a shining fracture. The second was also hard, but rather tough, and it had a brown hue ; the third, containing the smallest quantity of jelly, was of a bright reddish brown, and could be pulverized between the fingers. In order to establish more clearly the difference between these precipitates, they were subjected to the action of such reagents as might have the power of removing from them the excess of tan, and leave the compound in its most perfect state. This seemed to be effected by boiling the third precipitate in a large quantity of water, in consequence of which process the fluid was found to have acquired the property of copiously precipitating

precipitating jelly. The water exhibited a reddish tinge, and was also slightly affected by the addition of iron, showing, that it contained a minute portion of gallic acid; no effect was, however, produced on it by the muriate of tin. The boiled precipitate now approached in its appearance to the one which was composed of equal parts of tan and jelly; it was of a deeper colour and harder consistence. The first of these three precipitates was boiled in the same manner that this third had been, but the water was not in the least degree affected by jelly. It may appear singular, that any part of a substance, which had been precipitated from water, should be dissolved by it, but it probably depends upon the action of the greater mass of the fluid; and the fact is confirmed by Mr. Davy's remark, that the stronger the solutions are upon which we operate, the more completely will their solid contents be separated from them.

From the foregoing observations and experiments we may infer, that the method of detecting the quantity of jelly in any fluid, by the precipitate which it forms with tan, cannot be employed with any prospect of obtaining accurate results; nor can jelly be depended upon for the purpose of obtaining the amount of the tan in any astringent vegetable infusion. In the animal analysis this deficiency will probably be found of little importance; for, notwithstanding the proportion of jelly which enters into our solids, and which may be readily extracted from them by water, I am inclined to believe, that nothing, which is properly entitled to the name of jelly, will be found to exist in any of our fluids. When I first began these investigations I was induced to form a contrary opinion, and a contrary doctrine is maintained in our most valuable systematic works. I have, however, endeavoured to prove, that jelly is not found in the blood, where it has been supposed to exist in the largest quantity*; I do not find any trace of it in the albumen ovi, in the saliva, in the fluid of the hydrocephalus, of spina bifida, or of ascites, nor in the liquor amnii. By far the largest proportion of animal matter in all these fluids is albumen, existing sometimes in its coagulated, and sometimes

Accurate results not to be obtained in this process.

No jelly in the animal fluids,

but albumen,

* Medico-chirurgical Trans. V. I., p. 47.

and mucus? in its uncoagulated state. There appears, however, to be some animal substance beside the albumen, at least in the greatest part of them, to which I have hitherto assigned the name of mucus, but whether properly or not, must be the subject of future consideration.

I am, Sir,

Your obedient servant,

Liverpool, Aug. 3, 1809.

J. BOSTOCK.

II.

The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, &c. By HUMPHRY DAVY, Esq. Sec. R. S. F.R. S. Ed. and M. R. I. A.

(Continued from vol. XXIII, p. 334.)

6. Experiments on the Decomposition and Composition of the Boracic Acid.

Boracic acid decomposed.

IN the last Bakerian Lecture* I have given an account of an experiment, in which boracic acid appeared to be decomposed by Voltaic electricity, a dark coloured inflammable substance separating from it on the negative surface.

Attempt to effect this in quantities.

In the course of the spring and summer, I made many attempts to collect quantities of this substance for minute examination. When boracic acid, moistened with water, was exposed between two surfaces of platina, acted on by the full power of the battery of five hundred, an olive-brown matter immediately began to form on the negative surface, which gradually increased in thickness, and at last appeared almost black. It was permanent in water, but soluble with effervescence in warm nitrous acid. When heated to redness upon the platina it burnt slowly, and gave off white fumes, which slightly reddened moistened litmus paper; and it left a black mass, which, when examined by

* Phil. Trans. for 1808, p. 43; or Journal, vol. XX, p. 331.

the magnifier, appeared vitreous at the surface, and evidently contained a fixed acid.

These circumstances seemed distinctly to show the decomposition and recombination of the boracic acid; but as the peculiar combustible substance was a nonconductor of electricity, I was never able to obtain it, except in very thin films upon the platina. It was not possible to examine its properties minutely, or to determine its precise nature, or whether it was the pure boracic basis; I consequently endeavoured to apply other methods of decomposition, and to find other more unequivocal evidences upon this important chemical subject.

The combustible substance obtained only in thin films.

I have already laid before the Society an account of an experiment*, in which boracic acid, heated in contact with potassium in a gold tube, was converted into borate of potash, at the same time that a dark coloured matter, similar to that produced from the acid by electricity, was formed. About two months after this experiment had been made, namely, in the beginning of August, at a time that I was repeating the process, and examining minutely the results, I was informed, by a letter from Mr. Cadell at Paris, that Mr. Thenard was employed in the decomposition of the boracic acid by potassium, and that he had heated the two substances together in a copper tube, and had obtained borate of potash, and a peculiar matter concerning the nature of which no details were given in the communication†.

Boracic acid decomposed by Thenard.

That the same results must be obtained by the same methods of operating, there could be no doubt. The evidences for the decomposition of the boracic acid are easily gained; the synthetical proofs of its nature involve more complicated circumstances.

I found, that, when equal weights of potassium and boracic acid were heated together in a green glass tube, which had been exhausted after having been twice filled with hydrogen, there was a most intense ignition before the temperature was nearly raised to the red heat; the potassium entered into vivid inflammation, where it was in contact with

Potassium and boracic acid heated together.

* Phil. Trans. Part II, 1808, p. 343; or Journal, vol. XXI, p. 375.

† Gay Lussac and Thenard's paper is given in our last volume, p. 260.
the

the boracic acid. When this acid had been heated to whiteness, before it was introduced into the tube, and powdered and made use of while yet warm, the quantity of gas given out in the operation did not exceed twice the volume of the acid, and was hydrogen.

Large quantities could not be used. I could only use twelve or fourteen grains of each of the two substances in this mode of conducting the experiment; for when larger quantities were employed, the glass tube always ran into fusion from the intensity of the heat produced during the action.

Effect of naphtha. When the film of naphtha had not been carefully removed from the potassium, the mass appeared black throughout; but when this had been the case, the colour was of a dark olive-brown.

Proper proportion of the two. In several experiments, in which I used equal parts of the acid and metal, I found that there was always a great quantity of the former in the residuum, and by various trials, I ascertained that twenty grains of the potassium had their inflammability entirely destroyed by about eight grains of boracic acid.

Apparatus. For collecting considerable portions of the matters formed in the process, I used metallic tubes furnished with stop-cocks, and exhausted after being filled with hydrogen.

When tubes of brass or copper were employed, the heat was only raised to a dull red; but when iron tubes were used, it was pushed to whiteness. In all cases the acid was decomposed, and the products were scarcely different.

Results in a copper tube. When the result was taken out of a tube of brass or copper, it appeared as an olive coloured glass, having opaque, dull olive-brown specks diffused through it.

It gave a very slight effervescence with water, and partially dissolved in hot water, a dark olive coloured powder separating from it.

In an iron tube. The results from the iron tube, which had been much more strongly heated, were dark olive in some parts, and almost black in others. They did not effervesce with warm water, but were rapidly acted upon by it, and the particles separated by washing were of a shade of olive, so dark as to appear almost black on white paper.

Solutions. The solutions obtained, when passed through a filter, had
a faint

a faint olive tint, and contained subborate of potash, and potash. In cases when instead of water a weak solution of muriatic acid was used for separating the saline matter from the inflammable matter, the fluid came through the filter colourless.

In describing the properties of the new inflammable substance separated by washing, I shall speak of that collected from operations conducted in tubes of brass, in the manner that has been just mentioned; for it is in this way, that I have collected the largest quantities.

Largest quantities collected in brass tubes.

It appears as a pulverulent mass of the darkest shades of olive. It is perfectly opaque. It is very friable, and its powder does not scratch glass. It is a nonconductor of electricity.

Its properties.

When it has been dried only at 100° or 120° , it gives off moisture by increase of temperature; and, if heated in the atmosphere, takes fire at a temperature below the boiling point of olive oil, and burns with a red light and scintillations like charcoal.

Heated in air.

If it be excluded from air and heated to whiteness in a tube of platina, exhausted after having been filled with hydrogen, it is found very little altered after the process. Its colour is a little darker, and it is rather denser; but no indications are given of any part of it having undergone fusion, volatilization, or decomposition. Before the process its specific gravity is such, that it does not sink in sulphuric acid; but after, it rapidly falls to the bottom in this fluid.

Heated in vacuo.

The phenomena of its combustion are best witnessed in a retort filled with oxygen gas. When the bottom of the retort is gently heated by a spirit lamp, it throws off most vivid scintillations like those from the combustion of the bark of charcoal, and the mass burns with a brilliant light. A sublimate rises from it, which is boracic acid; and it becomes coated with a vitreous substance, which proves likewise to be boracic acid; and after this has been washed off, the residuum appears perfectly black, and requires a higher temperature for its inflammation than the olive coloured substance; and by its inflammation produces a fresh portion of boracic acid.

Its combustion in oxygen gas,

In oximuriatic acid gas the peculiar inflammable substance and in oximuriatic acid gas.

stance occasions some beautiful phenomena. When this gas is brought into contact with it at common temperatures, it instantly takes fire, and burns with a brilliant white light; a white substance coats the interior of the vessel in which the experiment is made, and the peculiar substance is found covered by a white film, which by washing affords boracic acid, and leaves a black matter, which is not spontaneously inflammable in a fresh portion of the gas; but which inflames in it by a gentle heat, and produces boracic acid.

Heated in hydrogen or nitrogen.

The peculiar inflammable substance, when heated nearly to redness in hydrogen, or nitrogen, did not seem to dissolve in these gasses, or to act upon them; it merely gained a darker shade of colour, and a little moisture rose from it, which condensed in the neck of the retort in which the experiment was made.

Its action on fluids containing oxygen:

On the fluid menstrua containing oxygen it produced effects, which might be looked for from the phenomena of its agency on gasses.

nitric acid,

When thrown into concentrated nitric acid, it rendered it bright red, so that nitrous gas was produced and absorbed; but it did not dissolve rapidly, till the acid was heated; when there was a considerable effervescence, the peculiar substance disappeared, nitrous gas was evolved, and the fluid afforded boracic acid.

sulphuric acid,

It did not act upon concentrated sulphuric acid, till heat was applied; it then produced a slight effervescence; the acid became black at its points of contact with the solid; and a deep brown solution was formed, which, when neutralized by potash, gave a black precipitate.

muriatic acid,

When heated in a strong solution of muriatic acid, it gave it a faint tint of green; but there was no vividness of action, or considerable solution.

& acetic acid.

On acetic acid heated it had no perceptible action.

It combined with fixed alkalis.

It combined with the fixed alkalis, both by fusion and aqueous solution, and formed pale olive coloured compounds, which gave dark precipitates when decomposed by muriatic acid.

Its action on sulphur,

When it was kept long in contact with sulphur in fusion, it slowly dissolved, and the sulphur acquired an olive tint.

phosphorus,

It was still less acted upon by phosphorus, and after an hour's

hour's exposure to it, had scarcely diminished in quantity, but the phosphorus had gained a tint of pale green.

It did not combine with mercury, when they were heated together and mercury.

These circumstances are sufficient to show, that the combustible substance obtained from boracic acid by the agency of potassium is different from any other known species of matter; and it seems, as far as the evidence extends, to be the same as that procured from it by electricity; and the two series of facts seem fully to establish the decomposition, and recomposition of the acid.

Differs from any known matter.

From the large quantity of potassium required to decompose a small quantity of the acid, it is evident that the boracic acid must contain a considerable proportion of oxygen. I have endeavoured to determine the relative weights of the peculiar inflammable matter and oxygen, which compose a given weight of boracic acid; and to this end I made several analytical and synthetical experiments; I shall give the results of the two, which I consider as most accurate.

Boracic acid contains much oxygen.

Twenty grains of boracic acid and thirty grains of potassium, were made to act upon each other by heat in a tube of brass; the result did not effervesce when washed with diluted muriatic acid; and there were obtained after the process, by slight lixiviation in warm water, two grains and about six sixteenths of the olive coloured matter. Now thirty grains of potassium would require about five grains of oxygen, to form thirty-five of potash; and according to this estimation, boracic acid must consist of about one of the peculiar inflammable substance, to nearly two of oxygen.

Apparently in one instance 2 p. oxygen to 1 base:

A grain of the inflammable substance in very fine powder, and diffused over a large surface, was set fire to in a retort, containing twelve cubical inches of oxygen; three cubical inches of gas were absorbed, and the black residuum, collected after the boracic acid had been dissolved, was found to equal five eighths of a grain. This, by a second combustion, was almost entirely converted into boracic acid, with the absorption of two cubical inches and one eighth more of oxygen. The thermometer in this experiment was at 58° Fahrenheit, and the barometer at 30.2.

in another

1·8 oxygen to
1 base.
Oxide with
24·8 per cent
of oxygen.

Sources of error both in
the analysis &
syntheses.

Is the base of
boracic acid
simple or com-
pounded?

The dark olive
substance most
probably a
compound.

Heated with
potassium.

Exposed to the
action of po-
tassium in
ether,

According to this result, boracic acid would consist of one of the inflammable matter to about 1·8 of oxygen; and the dark residual substance, supposing it to be simply the inflammable matter combined with less oxygen than is sufficient to constitute boracic acid, would be an oxide, consisting of about 4·7 of inflammable matter to 1·55 of oxygen.

These estimations, I do not however venture to give as entirely correct. In the analytical experiments, there are probably sources of error, from the solution of a part of the inflammable matter; and it possibly may retain alkali, which cannot be separated by the acid. In the synthetical process, in which washing is employed, and so small a quantity of matter used, the results are still less to be depended upon; they must be considered only as imperfect approximations.

From the general tenour of the facts it appears, that the combustible matter obtained from boracic acid bears the same relation to that substance, as sulphur and phosphorus do to the sulphuric and phosphoric acids. But is it an elementary inflammable body, the pure basis of the acid? or is it not, like sulphur and phosphorus, compounded?

Without entering into any discussion concerning ultimate elementary matter, there are many circumstances, which favour the idea, that the dark olive substance is not a simple body; its being nonconducting, its change of colour by being heated in hydrogen gas, and its power of combining with the alkalis; for these properties in general belong to primary compounds, that are known to contain oxygen.

I heated the olive coloured substance with potassium, there was a combination, but without any luminous appearance, and a gray metallic mass was formed; but from the effect of this upon water I could not affirm, that any oxygen had been added to the metal, the gas given off had a peculiar smell, and took up more oxygen by detonation than pure hydrogen, from which it seems probable, that it held some of the combustible matter in solution.

It occurred to me, that, if the pure inflammable basis were capable of being deoxygenated by potassium, it would probably possess a stronger affinity for oxygen than hydrogen, and therefore be again brought to its former state by water.

I made

I made another experiment on the operation of potassium on the olive coloured substance, and exposed the mixture to a small quantity of ether, hoping that this might contain only water enough to oxygenate the potassium; but the same result occurred as in the last case; and a combination of potash and the olive coloured substance was produced, insoluble in ether.

I covered a small globule of potassium with four or five times its weight of the olive coloured matter, in a platina tube exhausted, after being filled with hydrogen; and heated the mixture to whiteness: no gas was evolved. When the tube was cooled, naphtha was poured into it, and the result examined under naphtha. Its colour was of a dense black. It had a lustre scarcely inferior to that of plumbago. It was a conductor of electricity. A portion of it thrown into water occasioned a slight effervescence; and the solid matter, separated, appeared dark olive, and the water became slightly alkaline. Another portion examined, after being exposed to air for a few minutes, had lost its conducting power, was brown on the surface, and no longer produced an effervescence in water.

Some of the olive inflammable matter, with a little potassium, was heated to whiteness, covered with iron filings, and in contact with iron filings, a dark metalline mass was formed, which conducted electricity, and which produced a very slight effervescence in water, and gave by solution in nitric acid, oxide of iron and boracic acid.

The substance which enters into alloy with potassium, and with iron, I am inclined to consider as the true basis of the boracic acid. True basis of the acid.

In the olive coloured matter this basis seems to exist in union with a little oxygen; and when the olive coloured substance is dried at common temperatures, it likewise contains water. Olive coloured matter.

In the black nonconducting matter, produced in the combustion of the olive coloured substance, the basis is evidently combined with much more oxygen; and in its full state of oxygenation it constitutes boracic acid. Black matter.

From the colour of the oxides, and their solubility in alkalis, from their general powers of combination, and from the conducting The boracic basis probably metallic.

conducting nature and lustre of the matter produced by the action of a small quantity of potassium upon the olive coloured substance, and from all analogy ; there is strong reason to consider the boracic basis as metallic in its nature, and I venture to propose for it the name of *boracium*.

7. *Analytical Inquiries respecting Fluoric Acid.*

First experiments on fluoric acid gas.

I have already laid before the Society the account of my first experiments on the action of potassium on fluoric acid gas*.

I stated, that the metal burns when heated in this elastic fluid, and that there is a great absorption of the gas.

Since the time that this communication was made, I have carried on various processes, with the view of ascertaining accurately the products of combustion, and I shall now describe their results.

Fluoric acid gas introduced to potassium,

When fluoric acid gas, that has been procured in contact with glass, is introduced into a plate glass retort, exhausted after being filled with hydrogen gas, and containing potassium, white fumes are immediately perceived. The metal loses its splendour, and becomes covered with a grayish crust.

and heated.

When the bottom of the retort is gently heated, the fumes become more copious ; they continue for some time to be emitted, but at last cease altogether.

An addition of hydrogen to the gas.

If the gas is examined at this time, its volume is found to be a little increased, by the addition of a small quantity of hydrogen.

Second application of heat, and temperature raised.

No new fumes are produced by a second application of a low heat ; but when the temperature is raised nearly to the point of sublimation of potassium, the metal rises through the crust, becomes first of a copper colour and then of a bluish black, and soon after inflames and burns with a most brilliant red light.

* Phil. Trans., Part II, 1808, p. 343 ; [Journal vol. XXI, p. 375.] The combustion of potassium in fluoric acid I have since seen mentioned in the number of the *Moniteur*, already so often quoted, as observed by M. M. Gay Lussac and Thenard ; but no notice is taken of the results. [They are given in our present number, p. 29.]

After this combustion, either the whole or a part of the fluoric acid, according as the quantity of potassium is great or small, is found to be destroyed or absorbed. A mass of a chocolate colour remains at the bottom of the retort; and a sublimate, in some parts chocolate, and in others yellow, is found round the sides, and at the top of the retort.

Product of the combustion.

When the residual gas afforded by this operation is washed with water, and exposed to the action of an electrical spark mixed with oxygen gas, it detonates and affords a diminution, such as might be expected from hydrogen gas.

Residual gas, hydrogen.

The proportional quantity of this elastic fluid differs a little in different operations. When the fluoric acid has not been artificially dried, it amounts to one sixth or one seventh of the volume of the acid gas used; but when the fluoric acid has been long exposed to calcined sulphate of soda, it seldom amounts to one tenth.

Its proportion.

I have endeavoured to collect large quantities of the chocolate coloured substance for minute examination; but some difficulties occurred.

Attempt to collect large quantities of the product.

When I used from eighteen to twenty grains of potassium, in a retort containing from twenty to thirty cubical inches of fluoric acid gas, the intensity of the heat was such as to fuse the bottom of the retort, and destroy the results.

In a very thick plate glass retort, containing about nineteen cubical inches of gas, I once succeeded in making a decisive experiment on ten grains and a half of potassium, and I found, that about fourteen cubical inches of fluoric acid disappeared, and about two and a quarter of hydrogen gas were evolved. The barometer stood at 30.3, and the thermometer at 61° Fahrenheit; the gas had not been artificially dried. In this experiment there was very little sublimate; but the whole of the bottom of the retort was covered with a brown crust, and near the point of contact with the bottom, the substance was darker coloured, and approaching in its tint to black.

Successful one.

When the product was examined by a magnifier, it evidently appeared consisting of different kinds of matter: a blackish substance, a white, apparently saline substance, and a substance having different shades of brown and fawn colour.

Product a compound.

The

- A nonconductor.** The mass did not conduct electricity, and none of its parts could be separated, so as to examine as to this property.
- Action on water.** When a portion of it was thrown into water, it effervesced violently, and the gas evolved had some resemblance in smell to phosphuretted hydrogen, and was inflammable.
- Heated in contact with air,** When a part of the mass was heated in contact with air, it burnt slowly, lost its brown colour, and became a white saline mass.
- and in oxygen.** When heated in oxygen gas in a retort of plate glass, it absorbed a portion of oxygen, but burnt with difficulty, and required to be heated nearly to redness; and the light given out was similar to that produced by the combustion of liver of sulphur.
- Examination of the water,** The water which had acted upon a portion of it was examined; a number of chocolate coloured particles floated in it. When the solid matter was separated by the filter, the fluid was found to contain fluuate of potash, and potash.
- and of the residuum.** The solid residuum was heated in a small glass retort in oxygen gas; it burnt before it had attained a red heat, and became white. In this process oxygen was absorbed, and acid matter produced. The remainder possessed the properties of the substance formed from fluoric acid gas holding siliceous earth in solution, by the action of water.
- Experiments on small quantities not decisive as to the pure basis.** In experiments made upon the combustion of quantities of potassium equal to from six to eleven grains, the portion of matter separable from the water has amounted to a very small part of a grain only; and operating upon so minute a scale, I have not been able to gain fully decided evidence, that the inflammable part of it is the pure basis of the fluoric acid; but with respect to the decomposition of this body by potassium, and the existence of its basis at least combined with a smaller proportion of oxygen in the solid product generated, and the regeneration of the acid by the ignition of the product in oxygen gas, it is scarcely possible to entertain a doubt.
- Decomposition of the fluoric acid analogous to that of the** The decomposition of the fluoric acid by potassium seems analogous to that of the acids of sulphur and phosphorus. In neither of these cases is the pure basis, or even the basis

in its common form, evolved; but new compounds result, and in one case sulphurets, and sulphites, and in the other phosphurets, and phosphites of potash, are generated.

sulphuric and phosphoric.

As silex was always obtained during the combustion of the chocolate coloured substance obtained by lixiviation, it occurred to me, that this matter might be a result of the operation, and that the chocolate substance might be a compound of the siliceous and fluoric basis in a low state of oxidation with potash; and this idea is favoured by some trials that I made to separate silex from the mass, by boiling it in concentrated fluoric acid; the substance did not seem to be much altered by the process, and still gave silex by combustion.

Silex always obtained: perhaps a result of the operation.

I endeavoured to decompose fluoric acid gas in a perfectly dry state, and which contained no siliceous earth; and for this purpose I made a mixture of one hundred grains of dry boracic acid, and two hundred grains of fluor spar, and placed them in the bottom of an iron tube, having a stop-cock and a tube of safety attached to it.

Fluoric acid decomposed by the boracic,

The tube was inserted horizontally in a forge, and twenty grains of potassium, in a proper iron tray, introduced into that part of it where the heat was only suffered to rise to dull redness. The bottom of the tube was heated to whiteness, and the acid acted upon by the heated potassium, as it was generated. After the process was finished, the result in the tray was examined.

and made to act on potassium as it was generated.

It was in some parts black, and in others of a dark brown. It did not effervesce with water: and when lixivated, afforded a dark brown combustible mass, which did not conduct electricity, and which, when burnt in oxygen gas, afforded boracic and fluoric acid. It dissolved with violent effervescence in nitric acid; but did not inflame spontaneously in oximuriatic acid gas.

Product.

I have not as yet examined any of the other properties of this substance; but I am inclined to consider it as a compound of the olive coloured oxide of boracium, and an oxide of the fluoric basis.

A compound of the boracic and fluoric oxides.

In examining the dry fluoric acid gas, procured in a process similar to that which has been just described, it gave very evident marks of the presence of boracic acid.

The dry gas contains boracic acid.

As

Attempt to obtain the chocolate coloured substance

As the chocolate coloured substance is permanent in water, it occurred to me, that it might possibly be producible from concentrated liquid fluoric acid at the negative surface in the Voltaic circuit.

from the acid of fluor spar expelled by the sulphuric in leaden vessels.

I made the experiment with platina surfaces, from a battery of two hundred and fifty plates of six inches, on fluoric acid the densest that could be obtained by the distillation of fluor spar and concentrated sulphuric acid of commerce in vessels of lead. Oxygen and hydrogen were evolved, and a dark brown matter separated at the deoxidating surface; but the result of an operation conducted for many hours merely enabled me to ascertain, that it was combustible, and produced acid matter in combustion; but I cannot venture to draw the conclusion, that this acid was fluoric acid, and it was not impossible, that some sulphurous or sulphuric acid might likewise exist in the solution.

Olive coloured substance from boracic acid heated in fluoric gas.

I heated the olive coloured inflammable substance, obtained from the boracic acid, in common fluoric acid gas in a plate glass retort; the temperature was raised till the glass began to fuse; but no change, indicating a decomposition, took place.

Potassium and fluor spar heated in hydrogen.

I heated six grains of potassium with four grains of powdered fluor spar in a green glass tube filled with hydrogen; there was a slight ignition, a minute quantity of hydrogen gas was evolved, and a dark gray mass was produced, which acted upon water with much effervescence, but left no solid inflammable residuum.

(To be concluded in our next.)

III.

Remarks on the Boracic Acid, addressed to the first Class of the Institute, December the 19th, 1809 by F. R. CURAUDAU, Professor of Chemistry applicable to the Arts, and Member of several literary Societies.*

If boracic acid be decomposed by potassium,

THE process, by which Messrs. Thenard and Gay-Lussac have announced, that they decomposed the boracic acid,

* Journal de Physique, March, 1809, p. 256.

though

though the same as they made known the 21st of June last, has acquired a fresh interest, from the explanation they have given of the phenomena, that take place during the experiment*. In fact, if, agreeably to these chemists, boracic acid be decomposed by the alkaline metals, and lose its acid properties by the subtraction of the oxygen, which is admitted to enter into its composition, this conclusion must be formed, that potash is an oxygenized substance; and that the alkaline metals are not, as I think I have proved, a compound of alkali with hydrogen and carbon, or, if you please, with hydrogen solely. We must equally infer, that the silex, with which I have shown the alkaline metals may readily be decomposed†, is likewise an oxygenizing substance, which, instead of being the instrument of a decomposition, is decomposed itself. These points at least follow from the explanation they have given of the decomposition of the boracic acid. In this point of view however my experiment of the decomposition of the alkaline metals by means of silex is interesting, since it would prove this substance to be an oxide.

this is an oxide and not a compound of potash, hydrogen, and carbon: and silex is an oxygenizing substance.

However, as in admitting such an hypothesis we cannot explain all the phenomena observed during the decomposition of the boracic acid by means of the alkaline metals, I conceived it would not be amiss to make some fresh experiments on this subject; in order to ascertain on the one hand, whether it were true, that the boracic acid is an oxygenized substance; and to discover on the other, if possible, what became of the hydrogen and carbon of the alkaline metal, which disappear in this experiment, without Thenard and Gay Lussac having told us any thing of what they conjecture in this respect. The result of my labours I now submit to the examination of the class, hoping, that it will perceive in my zeal no other motive, than that of paying a fresh tribute to science.

This hypothesis does not explain all the phenomena.

Among the experiments I have attempted, the following particularly attracted my attention.

As boracic acid readily decomposes the metal of potash, Boracic acid

* See Journal, vol. XXIII, p. 260.

† See Art. VI of our present number.

I thought

should prevent the formation of potassium.

I thought, that, by adding this acid to a mixture capable of producing the metal of potash, it would not only prevent its production, but likewise be converted into the new substance obtained by decomposing the metal of potash by boracic acid.

It does so.

To prove how far this conjecture was well founded, I introduced into a gunbarrel the result of the detonation of six parts of vegetable charcoal, four of refined borax, and two of nitrate of potash. I afterward tried to extract the alkaline metal, but, as I had foreseen, no metal was disengaged.

The product lixiviated

When the matter was cold, I lixiviated it with a sufficient quantity of boiling water, in order to take up all the soluble substances it might contain. I afterward evaporated the solution till it was completely concentrated, and let it cool.

does not yield the whole of the borax,

Having obtained from the liquor only part of the borax I had employed; and the liquid itself, after having been slightly acidulated, yielding me but very little boracic acid; I concluded, that the surplus of this acid had remained combined with the charcoal, and must be in the state, in which Messrs. Thenard and Gay-Lussac found that, which they had treated with the metal of potash. What seemed still to support this opinion was, I observed the residuum of the calcination was of a black bottle green.

part of its acid having combined with charcoal.

The insoluble residuum treated with nitric acid,

To satisfy myself whether in fact the boracic acid were contained in the insoluble residuum, I poured on the coal, while still wet, a certain quantity of nitric acid. I afterward subjected the mixture to the action of a gentle heat. A brisk effervescence soon took place, which I ascribed to the oxygenation of the substance, that has been designated by the name of *bore**, which resumed its former state.

yielded the remaining proportion of boracic acid.

When the acid ceased to act on the residuum, I lixiviated the mixture. The liquors I obtained being afterward suitably evaporated, I obtained by cooling the remainder of the boracic acid, which was nearly the whole contained in the borax I had employed.

This experiment does not prove the boracic acid to

This experiment, which I have repeated several times with the same success, though it has a great analogy with that, in which the alkaline metal is made to act immediately on

* Journal, vol. XXIII, p. 262.

the boracic acid, is far from proving however, as has been contain oxigen asserted, that this acid is an oxigenized body. In fact, if it ^{gen.} were, why, when treated alone with charcoal, does it not experience the same decomposition, as when it is combined with an alkali? How too can the alkali, which according to the hypothesis is itself an oxide, promote the disoxigenation of another oxide? Should it not be on the contrary, from the very nature of my experiment, an obstacle so much the greater to the decomposition of the boracic acid; as all the acids when combined with a base are less adapted for decomposition? This experiment then must afford an instance of an anomaly so much the more striking, if it were to the oxigen that we must ascribe the action of the boracic acid on the metal of potash. It would equally involve a manifest contradiction, if we were to admit, that the new substance, into which the boracic acid is transformed, is more simple than the acid was, before its state was changed.

We see then, that the experiment of the decomposition of borate of soda by means of carbon is far from proving, that the new substance, which Messrs. Thenard and Gay-Lussac obtained from boracic acid, is the radical of that acid. We see too, it proves still less, that oxigen is one of the constituent principles of the alkalis, as the celebrated English chemist, Davy, continues to believe.

The substance called *bore* not the boracic radical,

and the alkalis not oxides.

Thus the experiments, from which I think I have demonstrated, that the alkaline metals are nothing but a compound of the alkali with hidrogen and carbon, acquire fresh force; and so that the facts, which would seem to be adverse to them, on the contrary confirm the deductions I have made from them.

The new metals compounds of hidrogen, carbon, and alkali.

For instance, does not the decomposition of the alkaline metals by boracic acid, instead of proving, that the phenomena observed during this experiment are to be ascribed to oxigen, on the contrary show, that this principle acts no part in it? and that it is rather a supracomposition of the boracic acid, than the loss of one of its principles, which occasions the new properties it acquires?

Their decomposition by boracic acid proves these.

However, if this supracomposition of the boracic acid be not admitted, how shall we explain why there is no hidrogen, or next to none, disengaged during the decomposition

Bore a supracomound.

of

of the alkaline metals by this acid? How too happens it on the hypothesis of its disoxygenation, that no water is produced? Can we admit the disoxygenation of one substance, and the dishydrogenation of another at the same time, without producing water sufficient to be collected; and have its weight calculated? Undoubtedly not. Thus, were this the only objection to the decomposition of boracic acid, it would suffice to prove, that the new state, in which this acid is obtained, is not owing to its disoxygenation. But as there are still many other objections, which the philosophy of the science suggests, we cannot do otherwise than consider the new substance, into which the boracic acid is converted, as a combination of this acid with the hydrogen and the carbon, that it has taken from the alkaline metal.

This accounts for the hydrogen appearing in no form.

According to this theory we find no difficulty [in explaining, why, during the action of the boracic acid on the metal of potash, neither water nor hydrogen is disengaged; while on the hypothesis of the disoxygenation of this acid, we know not what becomes of the hydrogen, which the alkaline metal must necessarily lose.

This explanation, independently of its accounting for all the phenomena, has the farther advantage of leading to a more simple definition of an important point in chemistry, on which the opinion of chemists is not yet thoroughly fixed.

Phenomena of the combustion of *bore*.

With respect to the phenomena exhibited by the combustion of the substance, that produces the boracic acid, they are owing to the oxygenation of the hydrogen and carbon, which this acid had abstracted from the metal of potash; so that by the subtraction of these two principles it becomes boracic acid again, as by the same subtraction the alkaline metal had again become an alkali.

Hydrogen and carbon more dense in *bore* than in potash.

If we consider too, that hydrogen and carbon, in their state of combination with boracic acid, are less oxygenizable than they were when combined with the alkali, every thing leads us to believe, that this arises from the two principles having acquired a fresh degree of condensation at the instant of their union with the boracic acid: and what appears to give some foundation to this conjecture is, that, at the moment when the combination takes place, the matter
instantaneously

instantaneously becomes incandescent, a state that announces a great emission of caloric, and consequently a sudden condensation of some principles.

I shall not terminate this note, without imparting to the institute a fact, that appears to me very important, but from which I shall refrain from drawing any inference. It is as follows.

I have observed, that in several experiments I made to decompose borate of soda by means of charcoal, metallic globules were produced, which appeared to be formed in the midst of the mixture: but as I found, that this metallic product was of the same nature as the vessel in which I made my experiments, I intend to repeat them in a tube of platina, in order to ascertain, whether those of iron, which I employed, did not concur in the formation of the metallic globules I obtained.

Metallic globules produced in a mixture of charcoal and borax,

However, this is not the only occasion, on which I have found similar globules. I had before remarked them in the mixtures I had made for the purpose of producing the alkaline metals with charcoal.

and in other cases.

IV:

Abstract of a Paper on the Decomposition and Properties of the Fluoric Acid, presented the 9th of January to the Mathematical Class of the Institute, by Messrs. GAY-LUSSAC and THENARD.*

MESSRS. Gay-Lussac and Thenard, having decomposed the boracic acid by means of the metal of potash†, could not but try the same method of decomposing the fluoric and muriatic acids, the constituent principles of which were not yet known. This they have effected with respect to the fluoric acid, and they now make public the principal results of their labours.

Potassium applied to the decomposition of fluoric acid.

* Journal de Physique, January 1809, p. 95. For Mr. Davy's experiments on the decomposition of fluoric acid, see p. 20, of our present number.

† See Journal, Vol. XXIII, p. 260.

Attempts to procure pure fluoric acid.

Gas procured from fluat of lime and boracic acid produces vapour with all gasses containing water.

No water precipitated from it by cold.

It has a great affinity for water.

Properties of watersaturated with it.

Our first care, say they, was to obtain pure fluoric acid : but as this acid exists only combined with lime, and no one has yet been able to separate it, without its entering into combination with some other body, we were obliged to make a great number of trials, that procured to us the advantage of observing several facts, the most remarkable of which are the following. When air is placed in contact with the fluoric gas disengaged from a redhot iron tube containing fluat of lime and glacial boracic acid, vapours are formed as dense as those arising from muriatic acid gas and ammonical gas. It produces the same with all the other gasses, except the muriatic acid gas, provided those gasses have not been dried. But it does not alter the transparency of any of them, if they have remained some time in contact with lime, or muriate of lime. In the first case, where there is a production of strong vapours, the volume of gas diminishes equally, and only a few hundredths at the temperature of 7° [44.6° F.]. In the second case, where the gasses retain their transparency, their bulk does not alter. Hence we may infer, that fluoric acid gas is an excellent mean for indicating the presence of hygrometrical water in gasses ; and that all contain some, except the muriatic acid gas, fluoric gas, and probably ammoniacal gas. For this reason, if we expose fluoric gas to a cold of 15° or 19° [5° above or 2.2° below 0 F.], we find no trace of liquid separated ; while on exposing sulphurous acid gas, carbonic acid gas, &c., to the same degree of cold, water is suddenly deposited.

The dense vapours, produced by fluoric gas in the gasses that contain hygrometrical water, announce in it a great affinity for this fluid : and indeed it is no exaggeration to say, that water can absorb more of it than of muriatic acid gas, and probably more than two thousand times its bulk. When water is thus saturated with it, it is limpid, fuming, and exceedingly caustic. About a fifth part of what it contains may be abstracted from it by heat ; but, do what we will, it is impossible to get more. It then resembles concentrated sulphuric acid : it has its causticity and appearance : like it its boiling point is much above that of water, and it condenses entirely in striæ, though it contains still

still perhaps sixteen hundred times its bulk of gas. Is it not hence extremely probable, if not even demonstrated, that the sulphuric and nitric acids would be in the state of gas, if they were pure? and that they are indebted for the liquid state, in which we see them, to the water they contain?

Sulphuric and nitric acids always contain water.

Though our fluoric gas has a great affinity for water, and contains none, since it is obtained from matters perfectly dry, &c.; yet it cannot dissolve or convert into gas the smallest quantity. We kept a quart of fluoric gas in contact with a single drop of water over mercury for several hours; and this drop, instead of disappearing, increased in size. Hence it is proved, that this gas cannot contain water in any manner, either in the hygrometrical state, or in a state of combination. Ammoniacal gas is precisely in the same situation, at least with respect to combined water.

Fluoric gas cannot dissolve water.

Ammoniacal gas similar.

But it is not the same with muriatic acid gas: this it is true contains no hygrometrical water, but it contains water intimately combined, as Messrs. Henri and Berthollet first showed. By passing muriatic gas, in a gentle heat, through litharge, melted and reduced to a coarse powder, we have accomplished the extraction of this water, and caused it to appear in streams. From the experiments we have made on the direct combination of a certain quantity of this acid with an excess of oxide of silver, it must form about a fourth of its weight.

Muriatic acid gas contains water in combination.

The other gasses do not comport themselves with water like the preceding. No one contains combined water, but all contain hygrometrical water. Hence it follows, that fluoric acid gas and ammoniacal gas contain neither hygrometrical water, nor combined water*: that muriatic acid gas contains no hygrometrical water, but does contain combined water; and that all the other gasses contain only hygrometrical water.

All other gasses contain water uncombined.

What is most striking in these results is to see, that muriatic acid gas contains water, and that the fluoric and ammoniacal gas contains no combined water; but Gay-Lussac and Thénard do not yet venture to affirm, that it contains no water in the hygrometrical state.

Proportions of water in muriatic acid gas,

* It is certain, that, from the experiments of Mr. Berthollet jun. ammoniacal gas contains no combined water; but Gay-Lussac and Thénard do not yet venture to affirm, that it contains no water in the hygrometrical state.

moniacal gasses contain none; and particularly to find, that the muriatic acid gas contains it in such proportions, that, if it were entirely decomposed by a metal, all the acid would be absorbed by the oxide, and converted into a metallic muriate. This, as we have satisfied ourselves, takes place, when muriatic acid is gradually and successively passed through several redhot gun barrels filled with iron turnings.

or rather the elements of water.

The more we reflect on all these phenomena, the more difficult we find it to account for them. Is it not possible however, that oxygen and hydrogen may be two of the constituent principles of muriatic acid, and that they are not in the state of water in it, but that this is formed when the acid enters into combination with bodies, so that in the muriates it is quite different from what it is in the state of gas? Be this as it may, it is certain, that all the muriates indecomposable by fire, and which contain little or no water, cannot be decomposed, even at a very high temperature, either by the vitreous acid phosphate of lime, or by the boracic acid; that thus the acid is retained with very great force in the muriates; and that sulphuric acid itself, if deprived of water, very probably could not decompose them. But we will quit this hypothesis, and return to an examination of the properties of our fluoric gas.

Action of fluoric gas on vegetable matter.

We have considered already its physical properties, its action on the air, on all the gasses, and on water. Let us now consider how it acts on vegetable matters. These it attacks at least as powerfully as the sulphuric acid; and, like this acid, appears to act on them by occasioning water to be formed, for it chars them. Thus it readily converts alcohol into an ether, which we purpose to investigate; and instantly blacken the driest paper, diffusing a vapour, which is owing to the water that is formed and absorbs it.

A very potent acid.

Every thing then demonstrates to us, that this fluoric gas is one of the most powerful acids, and that it is not inferior in strength and causticity to concentrated sulphuric acid; yet it has no action on glass. Hitherto we had supposed, that it was pure: but then we suspected that it contained something, which prevented its action on silex; and in fact

But it was a compound of the fluoric and boracic acids.

soon

been found, that it held in solution a pretty large quantity of boracic acid.

The fluoric acid arising from the decomposition of fluuate of lime by boracic acid not being pure, we attempted to prepare it by decomposing this salt by the acid phosphate of lime. We obtained but very little; and what we did obtain contained in the first place the small quantity of silix, that existed in our fluuate of lime, and secondly a certain portion of the acid phosphate of lime itself. What is remarkable in this process is, that, when we used a siliceous fluuate of lime, the decomposition of the salt was very rapid, in consequence of the action of the silix on fluoric acid, and gave rise to a great deal of siliceous fluoric gas.

Fluate of lime and acid phosphate of lime yielded a fluoric acid gas with silix in it.

Considering then, that the fluoric gas arising from the fluuate of lime and boracic acid contained no water, and was not capable of dissolving any, we thought, contrary to the generally received opinion, that the case would probably be the same with that prepared in leaden vessels by means of concentrated sulphuric acid.

Fluate of lime decomposed by sulphuric acid in leaden vessels

But instead of obtaining the acid in the state of gas by this means, we had it in a liquid state, and possessing the following properties. In the air it emits dense vapours: with water it heats and even enters suddenly into ebullition: it scarcely comes into contact with glass before it destroys its polish, heats strongly, boils, and is converted into siliceous gas. Of all its properties the most extraordinary is its action on the skin. It scarcely touches this when it disorganizes it. A white spot is immediately seen, a great pain is soon felt: the parts adjacent to the point touched speedily grow white and painful; and in a little time a blister is formed, covered by a thick white skin, and containing matter.

yielded only a liquid acid.

its properties.

Singular action on the skin.

However small the quantity the phenomena equally take place; only they proceed more slowly, so that sometimes they are not observed till seven or eight hours after the contact; and still the burn will be sufficiently severe, to cause acute pain, deprive the patient of sleep, and excite fever. The effects of these burns, as we are convinced by our own experience, may be stopped by the immediate application of a weak solution of caustic potash; which we know too, by

Remedy against burns.

experience, to be an excellent remedy against common burns.

Action of this liquid on potash.

It may readily be supposed, that we did not neglect to place such an active liquid in contact with the metal of potash. This experiment was made in a copper tube. At first we threw a piece the size of a small hazel nut into a small quantity of this liquid; and immediately a very loud detonation ensued, with a great evolution of light and heat. Afterward, desirous of knowing what was the cause of these phenomena, we caused the fluid to arrive at the metal gradually. In this way but little heat is produced, and we could collect the products of the experiment. These products were hydrogen, fluuate of potash, and water. Consequently this active liquid is a compound of fluoric acid and water.

It combines with all substances, and is the strongest of acids.

We see then, that this acid tends to combine with all substances, and that it forms with them solid, liquid, or gaseous compounds, according as it retains more or less elasticity, or expansive force. It is the only acid with which this is the case: and this property is even a proof, that it is the strongest and most active of acids.

Fluoric acid not obtainable pure.

Since we cannot in any way obtain fluoric acid pure, we can only study it when in combination with some substance. We must take it then combined with this or that substance, according to the result we wish to obtain.

Siliceous fluoric acid forms triple salts with alkalis, earths, and oxides.

If the object be to unite it with alkalis, earths, or metallic oxides, we must be careful not to employ siliceous fluoric acid, for in this case we should obtain triple salts. Thus, on pouring ammonia into acid fluuate of silex, we obtain a triple salt nearly insoluble, yet in great measure volatile. Thus too, on pouring muriate of barytes into acid fluuate of silex, we obtain after some time a crystalline precipitate, insoluble in a great excess of nitric acid, which might be mistaken for sulphate of barytes, and is nothing but fluuate of silex and barytes.

For decomposition the gas should be employed.

But when, instead of wanting to combine fluoric acid with these substances, we wish to decompose it, as we purposed to do by means of the metal of potash, it is evident, that we ought not to employ liquid fluoric acid, on account of the water present with it; and that we should prefer, either

ther the fluoric gas holding in solution boracic acid; or rather the siliceous fluoric gas, because the foreign matter in this, containing nothing combustible, cannot lead us into error, and can be of no injury farther than giving an addition of this matter. Accordingly we employed these gasses, and chiefly the siliceous fluoric gas, in our experiments on the decomposition of the fluoric acid, of which we shall now proceed to give an account.

When the metal of potash is placed in contact with siliceous fluoric gas at the common temperature, it undergoes no perceptible alteration, except becoming slightly dull on the surface: but if it be melted, it soon thickens, and burns vividly, with the extrication of much heat and light. In this combustion there is a great absorption of fluoric acid, very little hydrogen gas is disengaged, the metal disappears, and a solid substance of a reddish brown colour is produced.

Action of potassium on siliceous fluoric gas.

If this substance be treated with cold water, hydrogen gas is evolved, though it appears no longer to contain any metal. If, after having treated it with cold water, it is treated with hot, more hydrogen gas is evolved, but less than the first time; and on the whole scarcely a third as much as the metal itself would yield with water is obtained. If the waters of elutriation be added together and evaporated, we obtain from them nothing but fluuate of potash with excess of alkali; and if we examine the residuum, which, when well washed, is still of a reddish brown colour, we find it to possess the following properties. When thrown into a silver crucible at a cherry red heat, it burns vividly, and disengages a little acid gas; after which, from being insoluble in water, it becomes partly soluble. The portion that dissolves is fluuate of potash; that which does not dissolve is siliceous fluuate of potash.

The product treated with water.

Residuum burned in air,

If, instead of making the experiment in a crucible, it be done in a small bent glass jar filled with oxygen gas, and heated gradually, the inflammation is more vivid than in common air, a great quantity of oxygen is absorbed, and the gas that remains after the combustion is nothing but pure oxygen, with the addition of a little fluoric acid. The

and in oxygen gas.

product is solid, as in the preceding experiment, and is formed of fluato of potash and silex.

The fluoric gas is either decomposed, or combines with potassium without oxidizing it.

It is now evident, that, since little or no hydrogen gas is evolved on burning the metal of potash in fluoric acid gas, this combustion cannot be ascribed to water. Hence in this experiment either the fluoric acid is decomposed, or it combines with the metal without oxidizing it. These two hypotheses being the only ones that can be formed, let us discuss them in succession. If it were the metal, that combined entire with the fluoric acid, the probable result would be a very inflammable compound, which with water would give out as much hydrogen as the metal itself. But we obtain only a third of what ought to be evolved. Besides, a combination of this kind is contrary to all the facts on all possible suppositions; whether we consider the action of the fluoric acid on the metals and alkalis, or the action of the metal of potash on all the other acids. Hence we must conclude, that the fluoric acid is probably decomposed. Consequently in this decomposition must be formed a compound of the fluoric radical with potash and silex. It appears, that, when this radical is combined only with potash, it is capable of decomposing water like the phosphurets; but that, when it is combined with potash and silex, it does not decompose it, no doubt because this triple compound is insoluble.

It is probably decomposed.

Potassium easily burned in fluoric gas in small quantities,

Be this as it may, it is extremely easy, to effect the combustion of the metal of potash in fluoric gas. When we would burn only a small quantity of the metal, the operation may be performed very conveniently over mercury in a little glass vessel blown by a lamp, to the top of which the metal is conveyed on an iron rod, and which is heated by a burning coal till the inflammation commences.

or in large.

But if we would burn large quantities of the metal, the operation should be performed in a jar holding about a quart. This is first to be filled to within two fingers breadths with fluoric acid gas. The metal is then to be conveyed into it by means of an iron wire properly bent. A small capsule, which may be made of a crucible by removing a portion of the sides, being heated to a cherry red, is then to be introduced, holding it in a pair of tongs; and when

when it is emptied of the mercury by shaking it, the metal of potash is immediately to be placed in it, and it will presently burn with great force. The combustion being finished, and the capsule cooled, it is to be taken out, and the matter separated with a small spatula. This done, another portion of metal may be burned in this little capsule in the same jar; provided a quantity of fluoric acid, equal to what was absorbed in the first combustion, be passed up into it. A third and a fourth combustion may be accomplished in the same way. There is nothing to prevent this, since the jar may always be kept equally full of fluoric acid gas, and the metal is easily procured at pleasure, by following the process we have recommended. We will add, however, that for the complete success of these experiments, great care must be taken, to remove the oil from the surface of the metal with blotting paper; otherwise it will be decomposed, and give out a little hydrogen gas and carbon. In fact this inconvenience cannot be entirely avoided; and whatever precaution be taken, there is always a portion of oil interposed between the particles of the metal: but the quantity is so small, that it need not be regarded, and cannot be the source of any error in the results. To this oil is owing the property of rendering lime-water turbid, that the metals of potash and soda sometimes possess.

Care must be taken to free the potassium from oil.

V.

Description of a Process, by means of which Potash and Soda may be metallized without the Assistance of Iron; read before the French Institute the 18th of April, 1808; by F. R. CURAUBAU.*

THE decomposition of the alkalis, which I never considered as simple bodies, having long been an object of research with me, I was eager to repeat the experiment, in

Alkalis long supposed to be compounds.

* Journal de Physique, April 1808, p. 320.

which

Their metallization by means of iron does not always succeed. which Messrs. Thenard and Gay-Lussac announced potash and soda could be converted into metals by means of iron. Not having obtained more satisfactory results however than others, whom I have known to repeat the same experiments, I thought it right to pursue the researches I had already begun on the same subject, and the success of which appeared to me the more certain, as already the beautiful experiments of Mr. Davy had thrown great light on some phenomena, which I had observed, but which I could not before explain.

Is the prussiate of potash a compound of the metal with carbon? In fact, if, according to the hypothesis of the celebrated English chemist, potash and soda be metallic oxides, is it not more than probable, that the prussic calcinations are simply the combination of this metal with charcoal? Such at least was my opinion at that time; and it will appear how far it was well founded, since I have accomplished the metallization of potash and of soda, by heating strongly the alkali with charcoal, a process which, it is obvious, ranks among the prussic calcinations.

The metal of the fixed alkalis obtainable by two processes. The metallization of potash or soda taking place with either of the two mixtures I shall mention, and succeeding as well in stone retorts as in iron tubes, the first or second process may be employed indifferently. As to the nature of the vessel, I prefer iron, because it is more permeable to caloric, and less subject to fusion than the stone ware, particularly when the latter is penetrated with alkali; an inconvenience, that prevents the operation from being continued to the end, which does not happen so frequently with iron.

Process the first.

1st process. Mix intimately four parts of animal charcoal well powdered with three of carbonate of soda, dried on the fire without having been fused; and mix the whole with a sufficient quantity of linseed oil, but not so as to form a paste.

Process the second.

2d process. Take two parts of flour, and mix them intimately with one part of carbonate of soda prepared as in the preceding process,

process, and add to this mixture as much linseed oil as it will bear without ceasing to be pulverulent.

Whatever be the kind of vessel employed to calcine this matter, and whether it be the first or second mixture, we must always begin with heating it gradually: but as soon as the matter is obscurely red, the fire may be increased, till a fine sky blue light, surrounded with a greenish aureola, is perceived in the interior of the retort or iron tube. To this light will soon succeed a very copious vapour, which obscures all the interior of the vessel. This is the metal, which is disengaged from the mixture. The fire must then be urged no farther, for at this temperature the retort begins to fuse; and if the iron resist better, it is because the alkali penetrates it less readily than it does the stone ware, and likewise because the heat it receives is sooner transmitted to the matter within.

To collect the metal in proportion as it forms, introduce into the vacuum of the apparatus a rod of iron well cleaned; and, as it must not have time to grow red hot, take it out again in four or five seconds: it will then be found covered with metal, to remove which the rod is to be plunged instantly into a glass cucurbit filled with essence of turpentine. This cucurbit should be immersed in a tub of water, to prevent the essence from boiling; and notwithstanding this precaution, it will be heated so much sometimes as to take fire on the immersion of the iron rod.

Collection of
the metal.

To execute these processes well, three persons are necessary. One should take care of the fire and work the bellows. The most active should collect the metal as it is produced, and with the utmost celerity plunge the iron rods into the essence. The third must separate the metal that is on the rods, and then plunge them into water; not only to cool them, but also to remove the alkali, that may have escaped metallization, or been formed by combustion previous to the immersion of the metal in the essence of turpentine. He must likewise take care, to wipe the rods perfectly dry, that he who collects the metal may have nothing else to do.

Circumstances
necessary to
ensure success.

These processes, while the metal is producing, requires in the operators a dexterity not inferior to the celerity I have

have recommended. The attention of him who manages the bellows too is an object of no little importance; for, if he suffer the fire to slacken, the metal will immediately cease to be disengaged, and the rods will be covered with nothing but pure alkali. On the contrary, if he increase the fire at this period of the process, the apparatus will melt, and the experiment fail. This proves how high but uniform the temperature must be. I have observed, that the metal is always produced at the heat of melting iron. Accordingly an iron tube will seldom serve twice, and retorts always melt before the whole of the metal is obtained.

The metal a new compound.

I intend to inform the public of the observations, that I may hereafter make on this metallic product; but in the mean time I think I may conclude from my experiments, that the production of the metal is not owing, as has been said, to the disoxygenation of the alkali; but that it is a new compound, in which hydrogen appears to have entered into combination, and I conceive in a state of great condensation.

Hydrogen, alkali, and prussic radical given out.

Be this as it may, during the whole of the operation hydrogen, alkali not converted into metal, and prussic radical in the state of gas, continue to be disengaged. The last in particular I have collected in pretty considerable quantity.

Hydrogen an element of the metal or of charcoal.

These results tend to prove, that hydrogen is one of the component parts of the alkalis, the extrication of which is promoted by the charcoal; or that charcoal itself is a compound, one of the principles of which is hydrogen. There is no alternative, but one or the other of these hypotheses.

VI.

Observations and Experiments on the Nature of the New Properties of the Alkaline Metals: by the Same.*

Phenomena not explicable by supposing

SEVERAL of the phenomena, that accompany the metallization of potash and soda, being inexplicable on the hypothesis

* *Journal de Physique*, June, 1808, p. 452.

pothesis

pothesis of the alkalis being simply disoxygenized; and this theory besides agreeing neither with the properties of the oxygen, nor with those of ammonia, the principles of which should be analogous to those of potash and soda; I could not join in opinion with those chemists, who conceive the metallization of potash and soda to be merely the result of the disoxygenation of these substances. On the contrary, without prejudging any thing, I would consider only the facts; and in particular endeavour, if possible, to increase the series of those already known.

What rendered these researches still more interesting to me were the results of the experiments I had the honour to communicate to the class in the year 10 [1802 or 1803]; results that merited the attention of chemists the more, as the consequences I deduced from them predicted in some sort the possibility of metallizing the alkalis, the decomposition of which I announced.

Thus it is obvious, that Mr. Davy's discovery of the metallization of the alkalis by the galvanic pile could not fail to awaken in me the desire of being acquainted with these new products; and that, full of this subject, I should be one of the first to repeat the experiments announced for metallizing the alkalis, experiments in which I should have had the priority, had their publication been deferred another week.

Be this as it may, I have the satisfaction likewise of having discovered a process, which is peculiar to myself, and which succeeds in every laboratory; while I cannot say so much of the experiment I have repeated, since, whatever pains I have taken, I have been able to obtain only a ferruginous alkaline alloy.

It would be very desirable however, to learn where the difficulty lies, that every one may be enabled to repeat the experiment with equal success. What makes me particularly urgent for a knowledge of the means is, that, if it were proved to me, that the metal of the alkalis could be obtained separate by the assistance of iron, I should deduce this consequence from it; that the carbon, which enters into the composition of the alkaline metals, is one of the elements of

the alkalis to be simple oxides.

Decomposition of the alkalis announced long ago.

Mr. Davy's discovery excited the author's attention.

This process more generally successful than that of the other French chemists.

If the metal be obtained pure by means of iron, iron contains carbon.

of iron, which would tend to confirm the opinion I have given in my paper on the decomposition of the alkalis.

Two experiments demonstrate the presence of carbon in the new metals.

But I stop here, not to anticipate the question whether the metal of the alkalis contain carbon; for since I had the honour to address a note to the class, in which I mentioned two experiments, that appeared to me well fitted to demonstrate the presence of carbon in the alkaline metals, doubts on this important point have arisen, I request the class therefore, to allow me to make two experiments in its presence, against which I think nothing can be urged.

The first is the separation of the carbon contained in the metal of the alkalis without combustion: the second is the oxidation of the carbon, so as to convert it directly into carbonic acid.

That of hydrogen not so evident.

As to the hydrogen, it is not so easy to demonstrate its presence; particularly for one like me, who must be ten times in the right, to prove one truth.

The alkalis not being oxides the principal object.

However, if I demonstrate, that the alkalis are not oxygenized bodies, I shall have attained my object; and the question, whether hydrogen enter into the composition of the alkaline metals will be but a secondary consideration, which I propose to examine in another point of view.

I now proceed to the experiments, which may render us better acquainted with the nature and properties of the alkalis in the metallic form.

Grounds of the author's process.

Exp. 1. To prove the presence of carbon in the alkaline metals, it was necessary for me to have recourse to the action of a substance, with which the alkalis have more affinity, than they have with the principles that constitute them metals; and which at the same time should be incapable of furnishing any element, that would combine with those I sought to separate from the metallized alkalis. By these means I was sure of having the carbon separate, and thus furnishing a new proof, that the carbonic acid produced in burning the metal in lime-water arises from the oxygenation of the carbon.

Silex

Silex, from its indestructibility, the state of purity in which it is obtainable, and particularly its affinity for the alkalis, appeared to me to unite all the properties, that I wished

wished to find in the substance, which was to be employed in my experiment.

In fact, having heated silex in a glass tube with a little of the alkaline metal, it combined with the alkali, and set free the carbon. decomposes the metals, & sets charcoal free.

The carbon thus separated no longer took fire in the air; it required the assistance of heat.

Exp. 2. This experiment is that to which I alluded in the note I had the honour to address to the Class. It consists in enclosing in a thin bit of lead a ball of the metal of soda, and then immersing it in a vessel filled with lime-water. The metal thus confined is obliged to oxygenize itself at the expense of the oxygen of the water. Two affinities concur, to effect this decomposition: the first is that of the alkali for water, the second that of carbon for oxygen; an affinity so much the more energetic, as in this state the carbon exhibits to us a very remarkable instance of its great propensity to become oxidized; a propensity, which I shall refrain from explaining at present, for the consequences I should deduce from it would no doubt appear premature, considering the present state of our chemical knowledge. I therefore defer till another opportunity the communication of my ideas on this great and important question.

Sodium enclosed in lead, & immersed in limewater, is decomposed, & forms carbonic acid.

If in this second experiment I recommend taking the metal of soda, it is on account of its solidity, which allows it to be handled; and because its destruction is more slow, an advantage, that allows us to observe the phenomenon of the decomposition of water for some time. If, on the contrary, the experiment were made with the metal of potash, the decomposition of the water would be instantaneous; which, on the one hand, would oppose the combination of carbonic acid with lime-water, and on the other would force the gasses resulting from the decomposition of the metal to break the obstacles opposed to it by the lead, in which they would be included.

Sodium preferable to potassium for this experiment.

We see then, that the metal of potash is eminently combustible, and that of soda is less so; a property explicable by the difference of affinity of these alkalis for water.

Cause of their different properties.

One remark that I have made, and that will form the subject of a very curious experiment, is, that, in collecting the metal

Detonation of potassium in water.

metal of potash by means of iron rods, very loud detonations may be produced, the intensity of which is very similar to that of gunpowder employed in ten times the quantity.

Experiment. The following is the method of repeating this experiment with success. Instead of immersing the iron rods into essence of turpentine, the instant they are removed from the gun barrel to collect the metal, they must be plunged suddenly and perpendicularly into a bucket of water. An explosion will then take place, the loudness of which will be in proportion to the quantity of metal, and the diameter of the iron rod.

General conclusions.

From the experiments and observations I have had the honour of communicating to the class, it follows :

1st, That the conversion of the alkalis into metals is not a disoxygenation of those substances; and that, on the contrary, it is a combination of the alkalis with new elements.

2dly, That the affinity of the alkaline metals for oxygen is merely a chemical illusion, occasioned by a substance, the existence of which was not suspected.

3dly, That carbon is one of the constituent principles of the alkaline metals, since it can be obtained separate from them at pleasure, or converted into carbonic acid by oxygenation.

4thly, That, if the specific gravity of the alkaline metals be less than that of water, it is because hydrogen probably accompanies the carbon in this combination.

5thly, That the disoxygenation of substances, attempted to be effected by means of the alkaline metals, will always yield equivocal results, until we have a knowledge of all the elements, that compose these singular substances.

VII.

Improved Method of Forming Jury Masts: by Captain WILLIAM BOLTON, of the Royal Navy.*

SIR,

HEREWITH you will receive the model of a plan for fitting ships' jury masts, to be formed from the spare spars

Jury masts easily provided,

* Trans. of the Society of Arts, vol. XXVI, p. 167. The silver medal of the Society was voted to Captain Bolton for this improvement.

usually

usually carried on board King's ships, and in every merchantman that is properly found. By having jury masts so fitted, ships will be enabled to carry as much sail as on the usual regular mast; the great use of which I need not dwell on, only observing, that it may be of great importance to fleets after a general action, or when in want of proper lower masts, either at home or abroad, and enable ships, after the loss of their mast, to prosecute their voyage, or service, without any deficiency of sail.

I beg you will be pleased to lay it before the Society, and I have the honour to be,

Sir,

Your obedient humble servant,

WM. BOLTON.

REMARKS.

In the model in the Society's possession the main mast is broken about one third of its length above the deck, proper partners are secured on the deck, in which a hand mast and spare main top mast are fixed on each side of the broken main mast, and secured thereto by two spare caps, morticed on a square made in its centre. A strengthening cap, movable on these additional masts, connects them, and the upper parts of these masts are secured firmly by trustle trees in the main top. The foot of a spare fore topmast passes through a cap made from strong plank, morticed into the heads of the two temporary masts above mentioned, goes through the main top, and rests in the movable strengthening cap, which connects those two masts, and enables the fore topmast to be raised to any height which the main top will admit, and be then firmly secured by the upper cap, the main top, and the strengthening cap below it. The fore topmast being thus adjusted, the cross trees and topgallantmast are mounted upon it, which completes the whole business.

Two caps are the only things necessary to be made expressly for the purpose, the other articles being usually ready on board the ship.

In

Explanation of the plate. In Pl. I, figs. 1, 2, and 3, A A represent the partners or pieces of timber, which are bolted to the quarter deck for the mast to rest upon. B is the stump of the lower mast, which is cut square at the top, and of the same size as the head of the mast originally was; upon this square, the main and spare lower caps *a a* are fixed; two mortices must be cut in the partners A A to receive squares made at the lower ends of the two temporary masts D D, which are supported by the caps *a a*, one of them is a spare main topmast, the other a hand mast; these two support the main top E, additional squares being made on the tressel trees to receive each of them. *b* is a cap shown in fig. 2, made of four inch plank doubled for the purpose, and fitted upon the heads of the masts D D, for a fore topmast F F, the heel of which rests in a mortice made in the stump of the lower mast; it is also steadied by a double cap G, separately shown in fig. 3, on which it sits finally on the top. The topgallantmast H is fixed to the mast F by the top and cap in the usual manner. The figures 2 and 3 show the caps separated from the masts, and are the only things necessary to be made for the purpose; and the object of the cap, fig. 2, is to steady and to prevent any wringing of the lower jury masts, and to sit the topmast whenever it is reefed. The fore topmast F F appears in two separate pieces, on account of its length.

VIII.

An Improvement in the Construction of Anchors, to render them more durable and safe for Ships: with an improved Mode of Fishing Anchors. By Captain H. L. BALL, of the Royal Navy.*

SIR,

Anchor stocks **THE** great expense of timber in the navy for anchor stocks, and the frequency of their failing or giving way in
expensive and
frequently fail.

* Trans. of the Society of Arts, Vol. XXVI, p 170. The silver medal of the Society was voted to Capt. Ball for these improvements.

the

the centre, where the square of the anchor is let into the stock, have induced me to offer to the Society of Arts &c. a plan of an anchor, which may be cheaper in construction, and more likely to hold in various situations than those in common use.

The model I have sent will sufficiently explain my intention, and show how beneficial it may be in strengthening the anchor stocks. I wish much to notice to you its probability of holding in the ground longer than other anchors, on account of the additional weight of the stock; and this will more particularly be the case in banks which shelve suddenly down from the shore, such as at St. Helena, Cawsand Bay, and indeed in most of the islands in the West Indies. The proportion of additional iron, as explained by my model, is in all anchors to be twice and a half the diameter of the shank from each side at the stock, and of course this mode will supply the place of the present nuts, which are only intended to prevent the stock from slipping in and out, whenever it becomes loose, which accident anchors are very liable to in hot climates. My anchor stocks will save a considerable quantity of the finest timber, and give much greater security.

The improvement strengthens them and makes them hold better.

I likewise beg leave to offer to the Society a model of a double fish hook, for the purpose of fishing the anchor, an operation which, in the common mode of doing it, is frequently attended with accidents both to the ship and crew, from the anchor suddenly slipping unexpectedly in raising it to its proper position.

Accidents liable to happen in fishing anchors.

I flatter myself that these improvements will meet with the Society's approbation.

I am, Sir,

Your most obedient humble Servant,

Lower Mitcham,

H. L. BALL.

Feb. 13, 1808.

This anchor, in external appearance, differs very little from the common anchor; the improvement consists in the forming and fixing of the shank of the anchor to the stock. The stock *a a*, Pl. I. figs. 6 and 7, is made of two pieces of oak bolted together, and well secured by hoops. In the com-

The anchor described.

mon

mon method, in order to prevent the anchor stock from slipping off the shank, a square projection *b b*, fig. 8, is forged upon the shank; this is improved by Captain Ball, as shown in fig. 6, where this projection *dd* is extended on each side of the shank, far enough to receive two bolts through each of these extensions, which bolts hold firmly together the two pieces of timber that form the stock, and secure the stock fast to the shank. Two iron hoops, fig. 7, *ee*, are driven on the stock between the bolts, and *ffff* are other hoops, and *gggg* are treenails to strengthen the whole.

Improved method of fishing the anchor.

Fig. 4, represents Captain Ball's method of fishing an anchor. Fig. 5 shows an enlarged view of his double hooks used for this purpose.

In the usual operation of heaving an anchor, it is drawn up by the cable until it appears above water: the cable will not now raise it higher, it is therefore bowed up by the cat block *a*, fig. 4, from the cat head *b*, the cable *d* being slackened out as it rises. When it is got up as high as the cat block will raise it, a strong hook, called the fish hook, fastened to a rope *e*, which is suspended by a tackle from the shrouds, is hooked to the anchor at the bottom of the shank, and thus the arms of the anchor are elevated above the stock, until one of the flukes is brought up to the timber heads *ff*, to which it is made fast by a rope and chain, called the shank painter. In this operation the fish hook sometimes slips and occasions mischief, to remedy which, Captain Ball has applied two hooks instead of one, which keep firmer hold. These hooks are shown upon an enlarged scale at *gg* fig. 5, attached to the rope *e*; each of these hooks takes one of the arms of the anchor, close to the shank, and holds it firmly. *ii* are two small lines made fast to the hooks, to direct them so as to get proper hold of the anchor.

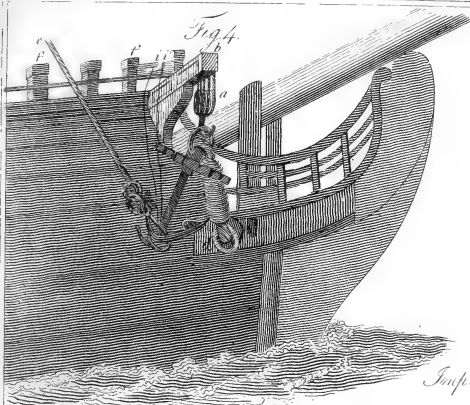
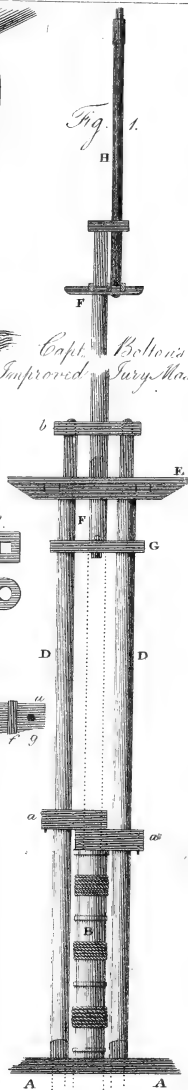


Fig. 4.

Fig. 1.

Capt. Bolton's Improved Jury Mast.



Capt. Ball's method of Fishing an Anchor.



Fig. 5.

Fig. 2.



Fig. 3.

Fig. 6.

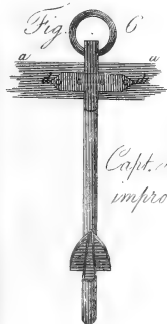
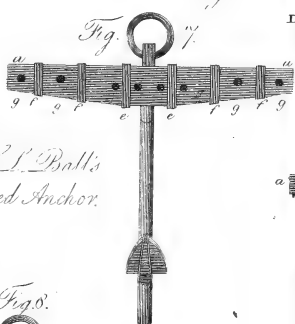


Fig. 7.



Capt. M. L. Ball's improved Anchor.

Fig. 8.

Common Anchor.



IX.

Observations on the Progress of Bodies floating in a Stream: with an Account of some Experiments made in the River Thames, with a View to discover a Method for ascertaining the Direction of Currents. By JAMES BURNEY, Esq.

HAVING frequently noticed, that the heavy craft on the River Thames, during a calm and without the assistance of oars or of towing, made a progress faster than the stream of the tide on the surface, it led me to make inquiry as well into the fact as concerning the cause, and gave rise to some experiments, which, with the ideas they suggested, are here set down; no otherwise according to method than being in the order they occurred.

Heavy bodies float down a stream faster than the current.

On questioning the men belonging to several barges, which, unaided by wind, oar, or towing, were floating with and overtaking the stream, they all agreed in the general fact, as a circumstance familiar to them. They said likewise, that a laden barge made greater progress than a light barge; and this was corroborated by the evidence of the boats attached to them being drawn after them; for the barges overtook the moving water so fast as to have good steerage way. They attributed the difference in favour of a laden barge, to her having (as they expressed it) more hold of the tide than a light barge: by which it appears, that they supposed the stream of the tide was stronger underneath than on the surface. Adhesion to the atmosphere may retard the surface, except when the current of the atmosphere (the wind) goes in the same direction with the current of the tide; and then it may occasion an acceleration.

Laden barges float faster than light ones.

Monday, July the 18th, I went on board a barge half laden, which was floating down the river, but with steerage way, between Putney and Chelsea bridges. I conjectured the rate of the tide to be a mile and a half per hour: there was a very light air of wind in a direction contrary to the stream.

This is not owing to a more rapid under current.

stream of the tide: but the barge, without any assistance of oars or towing, passed on, overtaking the stream, and her boat was towing astern. I fastened a riband to the end of a stick, and immersed it in the water about 20 inches, which was as low as the lowest part of the barge's bottom, and therefore sufficient to have shown, by the direction of the streamer, if the barge had been impelled forward by superior velocity of the under current, as in that case the streamer would have gone before the stick; but the streamer tended towards the stern, and was drawn after the stick: whence it was evident, that the barge's progress exceeded that of the stream underneath as well as on the surface, and that this excess was acceleration produced by some other cause*.

The surface of a stream an inclined plane.

As by the general law of gravitation the heaviest bodies descend with most velocity in a yielding medium, so it appears to be with bodies floating in a stream. The surface of a stream or current of water is not horizontal, but an inclined plane, and the inclination of the surface produces the current. Thus, when, by the attraction of the Sun or Moon, the sea is raised in some parts, it becomes depressed in others, and the water, seeking to regain its level, flows in a current from the superior parts.

The barges on the river in a calm therefore *slide downward* with the stream, and also on it.

Wherry outstripped by a barge.

A friend of mine in a wherry going to pass under London bridge, being closely preceded by a coal barge, was apprehensive of receiving damage from collision with the barge when under the bridge; but the waterman said the barge would shoot far enough ahead when she came to the indraught of the arch. And it happened accordingly; for

Under currents produced by local circumstances.

* No part of what is here said contradicts any received hypothesis concerning under currents. Some under currents proceed from visible causes, as when the wind blows for a length of time in one direction towards a coast, especially if it is an embayed coast, whereby the waters are accumulated and the surface near the shore is raised above the general level, till the pressure of the increased weight forces back the water underneath. Under currents, where the causes are not visible, may be supposed to be caused by inequalities in the bottom, in the same manner as eddies are caused by the projecting points of a coast interrupting the general course of a stream.

the

the barge, arriving first within the sterling heads, shot away from the wherry about 200 yards, by the superior momentum she acquired in the increased declivity.

A pressure perpendicular to the horizon applied to a body floating on a horizontal surface acts as increase of weight, having the effect only of making the body to which it is applied swim deeper, or occupy more space in the water. An oblique pressure, not strong enough to submerge the body, affects it in two directions; one downwards in the manner of weight, to which the body yields to a certain and definite extent; the other horizontal, in which direction the body continually gives way to the pressure. Almost every person has experienced the readiness of a boat to glide from under him, on putting his first foot in her. These two effects of an inclined pressure are separately in proportion to the whole pressure, one as the sine, the other as the cosine, of the angle of incidence is to radius.

If to a body floating on a horizontal surface a pressure is applied in a direction making with the horizon an angle of $89^{\circ} 59'$, the proportion of the pressure which would act horizontally is to the whole pressure, as the sine of $1'$ is to radius. And this proportion is $\frac{1}{100000}$ of the whole pressure. In like manner, if the surface inclines $1'$ from the true horizontal level, weight applied to a body floating on that surface will give an impulse towards the declining part of the surface equal to $\frac{1}{100000}$ of the weight applied. Consequently, a barge having in her 100 tons weight, floating with the stream where the declivity of the surface is $1'$, will receive an impulse towards the declining part of the surface equal to nearly 65lbs. which is little short of what is estimated to be the average pull of a horse.

Hence it seems naturally to follow, that two pieces of wood, equal in size but differing in weight, being placed in the water near to each other, would show if there was a current, by the heavier wood separating from the lighter in the direction of the stream. Likewise, that the quantity of separation in a given time might afford a measure for the strength of the current. And it is probable, that this would be found true in a smooth and equal running stream, where no interruption was caused by the wind.

Experiment
with an oak &
fir ball:

I supposed, that the best form to be given the wood for making the experiment would be globular, as being less liable than any other to be affected by irregularities in the surface of the water. I caused two wooden balls to be made, one of oak 6 inches in diameter, the other of fir, and not so large. I chose a time when the quietness of the air was next to calm, and the surface of the water very smooth. The balls were put into the stream; the oak swam deep, leaving a very small portion uncovered; but the fir ball was found so very susceptible of motion from the lightest air of wind, that no conclusion could be obtained from this experiment.

with a staff
loaded at one
end.

It was suggested by Mr. Rickman, my associate in these experiments, and whose observations jointly with my own have furnished this paper, that for showing the direction of the current, a long staff of light wood, loaded at one end, might better answer the purpose than two unconnected floating bodies, because whenever it got out of the right direction it would have a tendency to correct itself.

On Wednesday, July 27th, we made some experiments on the river; but the weather was not favourable. Two sticks, one of them a common walking stick with a piece of lead fastened to one end, the other a hollow tube (a joint of a fishing rod) loaded internally at one end, were put into the stream (but not in any preconcerted or remarked direction) and they both took the direction of the stream, the heaviest end becoming the most advanced. They were taken up, and being again put into the water in a direction opposite to the stream, they gradually regained their former direction: in what time was not observed. In endeavouring again to repeat the experiment, two barges passing caused us to lose sight of our sticks, and we did not find them afterwards.

Passage of boats
through the
arch of a bridge.

About an hour after the flood had made through London bridge, I noticed from the top of the bridge the passage of some of the craft. When any one drew near the arch, she did not keep pace with the water before her, so that on looking only at her head, she seemed to have stern way; but at her stern she left the marks of her track behind her. Two barges and a small boat, the small boat being in the middle

middle, at small and nearly equal intervals, followed each other through. That which first came to the increased fall under the arch, being precipitated, left the others far behind, till in their turn they were in like manner precipitated. When they arrived out of the rapid part of the stream into the smooth water, I did not observe, that their relative position was altered from what it had been before they came to the bridge: but the small boat had made use of oars.

It would answer other purposes than that of curiosity, to ascertain and form tables of the declivity of the surface at different velocities of current. The observations of altitudes at sea must be affected by currents, one part of the sensible horizon being higher than the other. A ship stationed in a tide which runs one way North, the other South, may expect to find the observed latitude vary with the tide.

Altitude of the horizon differing with the tide.

In the afternoon of the same day that we made the experiment with the sticks (the 27th), we made a very imperfect attempt to discover what was the declivity of the surface above Westminster bridge, or rather what angle the plane of the surface on the flood made with it on the ebb, by marking at two distant stations at the same times, the height of the water during the flowing, and likewise during the ebbing of the tide. One station was at one of the posts close under the Speaker's garden: the other, on the same side of the river, at the ferry opposite Cumberland Gardens. The distance between the stations, according to the maps of London, is seven furlongs.

Attempt to ascertain the declivity of the surface of the Thames.

At the post near the Speaker's garden, the difference of the height of the river, taken at 4 P. M., the tide then flowing, and at 7 P. M., the tide ebbing, was 25.5 inches.

At the ferry opposite Cumberland Gardens, the difference of the heights taken at the times above mentioned was 17.3 inches.

The tide was lower at 7 o'clock than at 4. The stream at each of those times was running at a rate which we conjectured to be nearly three miles per hour: therefore a greater variation was expected than 8.2 inches, which in a distance of 7 furlongs will give only 30" for the angular difference between the plane of the surface on the flood, and the

the plane on the ebb; so that the angle of declivity from the horizontal level, supposing it equal each way, was not more than 15°. Perhaps the difference would have been found greater, if the marks had been taken in mid stream, instead of close to the side of the river. The stoppage of Westminster bridge may likewise be supposed to occasion some swell in the part of the river immediately above it, during the ebb tide.

Experiments with sticks less satisfactory than before.

On Wednesday, August the 3d, we again made experiments with sticks; which proved less satisfactory than those we had before made. But, previous to describing farther operations, it is necessary to notice a consideration, which, when it first occurred, seemed an insurmountable objection to deriving any benefit from them. This was, the great difference between the surface in a river, and the surface in an open sea; so that an experiment, which might be found to succeed in the one, might scarcely be at all practicable in the other. To this objection it is reasonable to answer, or at least a reasonable encouragement to expect, that if a small stick will point the direction of the stream in a river, a long pole (a steering sail boom, for instance), will, in circumstances tolerably favourable, do so at sea. It seems within the rule of just proportion, that a spar as large as a steering sail boom as much exceeds a walking stick, as the irregularity of the surface at sea, in temperate weather, exceeds that of a river. In the experiment, two or more might be put into the sea at the same time, and if they agreed, there would be the greater reason for placing reliance on the result.

Long poles requisite at sea.

Experiment with heavy wood a foot beneath the surface.

Our next trial was with one of the South Sea island clubs, of a wood not buoyant, about three feet in length, and gradually tapering. It was buoyed at each end with cork, but with string enough to let it be about a foot under the surface; and that the corks at each end might be equally exposed to the air, they were so managed as to show equally and similarly above water. There is reason, however, to think it would have been more proper to have allowed the exposure to the air at each end to be proportioned to the weight sustained. In the manner the experiment was made, the club, being left to itself in the stream, did not take or keep

keep to any determinate direction. It was unfavourable to this experiment, that the club was not of greater length, and, perhaps, that its weight at each end was in the same proportion to its size.

We again tried with sticks loaded at one end. But the most that can be said of the results of this day's experiments is, that the loaded end evidently showed the most tendency to be downward with the stream. The sticks veered in their direction more than we had observed in the former trials. This brought to recollection a circumstance, to which we had not paid attention. The barges we had seen over-sliding the stream in a calm were kept in the right direction by their helm. Supposing a barge to be loaded at one end, and light at the other; without the help of the helm, the loaded end would probably not be found to keep her in the same direction with the stream, any more than the head sails only of a ship, being set, will, without the help of the helm, keep the ship before the wind.

This consideration leads again to trials with two or more separate and unconnected bodies. An experiment which can easily be made in a ship, is with a hogshhead, which can be filled after it is put into the sea, and a quart keg, which if the air should be quite calm it would be sufficient to half fill. This would approach the proportion of a barge and her small boat: but as no guidance can be given, the most regular shape (globular) seems the best.

The stream in the Thames above the bridges, from the unevenness and shallowness of the bottom, is unfavourable to experiments of the kind here recommended; and the superior convenience possessed by those whose constant occupation is on the waters, who have opportunities, without an hour's expense of time, to make experiments, which to other persons would cost days, have been inducements to publish the inquiry in its present state, to give it the best chance of being prosecuted with any effect.

JAMES BURNEY.

*James Street, Westminster,
August 20th, 1808.*

The

The above paper was read at a meeting of the Royal Society, February 16th, 1809. In consequence of some observations which it produced, the following remarks, in addition, were presented, and read at the Society:

Weight impels the stream itself, as well as the body floating on it.

In a paper I had the honour to present to the Royal Society, on the progress made by some bodies floating in a stream, they descending faster than the stream itself, I endeavoured to show, that this progress was the effect of perpendicular pressure, producing impulse towards the declining part of the surface. The same cause, indeed, evidently applies to the production of the stream itself; consequently the surface, and whatsoever floats on it, are, in this respect, on an equal footing, and the whole agree in pressing forward and in opposing resistance to whatsoever endeavours to overtake them. Without some auxiliary cause, therefore, a floating body cannot overtake the stream.

Shape and direction of a body affects its velocity.

The different shapes of bodies, and likewise the directions in which they are placed with respect to the direction of the impelling power, expose them to more or less resistance. A barge floating crossways to the stream receives the progressive impulse with the least advantage, her whole length acting in resistance to her overtaking the stream. The same barge when endways with the stream, is acted upon by the same quantity of impelling power, and her progress is opposed by less resistance.

Weight adds more to the impulse than to the resistance.

With increase of weight, both the impelling power and the resistance are increased: but when the barge is lengthways with the line of the stream, weight added will increase the impelling power in a greater proportion than the resistance is increased. Hence the heavy barge in a calm will overtake the light barge.

This short explanation I beg to offer as an addition to my former remarks on the subject, and shall be glad if it assists in any satisfactory manner to account for vessels overtaking the stream in a calm.

May 15th, 1809.

X.

New Method proposed for measuring a Ship's Rate of Sailing. By the same Gentleman.

A Line towing astern of a vessel, which is passing through the water, will pull against her head-way. As the ship's way increases, the pull of the line will increase; and *vice versa*. If this, with a proper scope of line (about 25 fathoms may probably be sufficient) shall be found to be a regulated quantity of pull corresponding in the same manner at all times to the rate of sailing, it will answer the purpose of a log. Many experiments have been made upon the same principle; but the most plain and easy one, of towing a measured length of line, has escaped trial; though less liable to give erroneous or variable results than any which can be made near a ship. By it, the rate of sailing may be obtained either constantly or occasionally, and can be taken with ease by one person: in which respect it would have great advantage over the common log, the use of which requires three persons.

By a trial made in a boat with about 20 fathoms of line, rather larger than log line, towing astern and fastened to a spring steelyard, the strength of the pull was found to vary with the rate of sailing, which however was not ascertained by measurement; but by estimation, the boat's rate of sailing during the trial varied between $2\frac{1}{2}$ knots and 5 knots per hour, and the pull of the line upon the steelyards was observed to vary from 2 lbs. to $5\frac{1}{2}$ lbs.; increasing and decreasing with the velocity. So great a variation in the strength of the pull gives all the advantage, which can be desired for forming a scale, and will allow of the experiment being made with smaller line.

If the proposed length of line is passed through a pulley so as to go clear out at the stern port or cabin window, and the inner end is fastened to a loose chain, of weight adapted to the purpose, on the deck under the pulley; or to a number of small weights made consecutive by short intervals of line, the chain or weights will be drawn up more or less according

A line towed astern of a ship will be a perpetual log.

The velocity indicated by weights,

cording to the ship's velocity. By a few comparisons of the quantity of weight raised from the deck with the rate of sailing, a scale may be marked.

or a spring and
index.

In an improved state of the experiment, instead of using weights or a pulley, the inner end of the line (coming direct from the water) can be fastened to a spring, and communicate with an index that shall express the rate of sailing.

This machine (if so plain a contrivance deserve that name) may be put on constant duty, or dropped occasionally to ascertain the rate.

Objections which occur, are,

Objections, &
answers to
them.

1st. The line being liable to contraction or expansion as the temperature of the water varies. But it is scarcely to be supposed, that the greatest contraction or expansion of line from its mean state (after it has been properly stretched and seasoned) will occasion an alteration of a hundredth part in the force of the pull.

2d. That in a fresh wind the part of the line between the ship and the surface of the water, will be liable to some additional pull from being exposed to the wind. To this inconvenience, the log line in the common way of heaving the log is likewise exposed when the wind is much aft. In either case, when the ship is not right before the wind, the remedy is the same: which is, to throw the log or the line over from before the lee gangway, and to give a few fathoms more of stray line; for which however, in the new method proposed, it would be necessary to apply a correction, the quantity of which may be accurately ascertained.

3d. The motion of a ship in pitching. But this is not to be regarded as an objection; for the rate of sailing is to be estimated only by what the experiment shows when the ship is going steadily; in the same manner as in taking bearings, if the compass swings, we wait till it is quiet. Whenever the ship goes steadily for ten seconds together, or even five seconds, the pull of the line will be regulated to the average rate of sailing.

XI.

Method of preventing Doors from Dragging on Carpets, or admitting Air underneath them. By Mr. JOHN TAD.*

SIR,

I HAVE taken the liberty of laying before the Society a Method of preventing air-tight doors from dragging on carpets. model of my invention to prevent doors from dragging on carpets, and to keep out the current of cold air, which enters under such doors as are not close to the carpets underneath them.

I can affix this machinery to the bottom of any door, so that the door shall pass over the carpet with ease, and, when shut, be air tight. It obviates the necessity of screw rising hinges, and is less expensive than other inventions for the same purpose.

The machinery is constructed of a slip of well seasoned beech wood, equal in length to the width of the door; this slip is one inch and a quarter wide, and half an inch thick, and to be covered with green cloth on the inside; it is to be hung to the bottom of the door with three small brass hinges, and is drawn up by a concealed spring as the door opens, and is forced down when the door shuts, by one end of it, which is semicircular, pressing upon a concave semicircular piece of hard beech wood, fastened at the bottom of the door case, and which holds it down close to the floor or carpet, so as to exclude the air from entering under it. Hoping this invention will meet with the approbation of the Society, I remain, with respect,

Sir,

Your most humble Servant,

No. 4, Little Hermitage Street,

JOHN TAD.

Wapping, Nov. 24, 1807.

A Certificate was received from Mr. William French, Certificate of its efficacy. No. 280, Wapping, stating, that John Tad had fixed to two of his room doors the invention above mentioned, and

* Trans. of the Society of Arts, vol. XXVI, p. 196. Five guineas were voted to Mr. Tad, for this communication.

that

that he found it to answer to his satisfaction, both in permitting the doors to pass clear of the carpets, and in keeping out the air.

The method
described.

Mr. Tad's invention consists in first cutting away the bottom of the door, so that it is about one inch and a quarter above the floor; this allows a sufficiency of room for the door to open over any carpet. To close the opening which would now be left under the door when shut, he proposes to fix beneath the door, by means of hinges, a slip of wood, of which *a b d e*, figs. 2 and 3, Plate II, is a section. Fig. 1 is a perspective view of the bottom of a door, with the invention annexed to it; fig. 2 is a section across the door when closed; fig. 3 is a view of the edge of the door when open; and fig. 4 is a section supposed to be made by cutting the door in two parts, edgeways. The hinges on which the slip turns, are fixed to the edge. In figs. 2 and 3, from *a* to *b* is exactly one inch and a quarter, so that when the ruler is turned down upon the hinges, it reaches the floor *A A*, as in fig. 2; in the other direction *ad* it is much less, being only half an inch, so that when it is turned up under the door, as in fig. 3, it leaves three quarters of an inch clear of the floor. It now remains to show how the ruler is turned up or down; it has always a tendency to rise up into the state of fig. 3, by the action of a steel wire spring, shown in figs. 2 and 4, which is concealed in a rebate cut in the bottom of the door; one end of the wire is screwed fast to the door at *f*, the other is inserted into an eye fastened into the slip at *g*. To throw it down into the position of figs. 2 and 4, the end *h*, fig. 4, of the slip farthest from the hinges of the door, is cut into a semicircle, as seen in fig. 3. When the door is just closed, this semicircle is received into a fixed concave semicircle *k*, fig. 3, cut in the end of a piece of wood *k l*, made fast to the door case; the line *m l*, fig. 3, represents the plane of the door when shut, and *p p* part of the door seen edgeways; as the door in shutting moves from *p* to *m*, the semicircular end of the slip *a b d e* presses against the end of the piece *k l*, and as the door proceeds, it turns down as in fig. 2, so that by the time the door is shut, the slip is turned quite down; the edge *e b* of the slip is cut into a
segment

segment of a circle struck from the hinges on which it turns. The perspective view in fig. 1 shows that this contrivance, applied to any door, will not offend the eye, as it can scarcely be distinguished from an ordinary door. *K*, fig. 1, shows the concave semicircle of the piece of wood fastened to the doorcase, in which the semicircular end of the slip *c* is to be received.

XII.

Description of an improved Screw-wrench, to fit different sized Nuts, or Heads of Screws. By Mr. WILLIAM BARLOW.*

SIR,

PERMIT me to make a few observations on a shifting screw-wrench of my invention, which I beg leave to lay before the Society of Arts &c. through the hands of Mr. Brunel, inventor of the block machinery here.

I have found, from long experience, the imperfections of the various wrenches in common use, for the screw heads and nuts of engines in general, which are often materially injured for want of an instrument that would fit variety of sizes, and be applied with as much advantage as a solid wrench. I have had it in view to unite steadiness with conveniency in making such an instrument, and flattering myself that I have obtained both, I am desirous to communicate my invention to the Society, and have therefore sent an instrument on the principle I have actually used, and which has met with the approbation of my employers and other persons.

This wrench, by means of a nut and screw, is adjusted with the greatest ease to the exact size required, and in that state rendered so steady, that in use it is found equal to a solid wrench.

* Trans. of the Society of Arts, vol. XXVI, p. 199. Five guineas were voted to Mr. Barlow for this invention.

I have

I have, for several years, been intrusted with the care and repairs of many valuable engines of various descriptions, composing the block machinery in this dock-yard, and I have always considered it as an object of great importance, for the preservation and neat appearance of engines, to attend to all the means which would obtain these advantages, and such, I think would arise from the use of my universal wrench.

May be made
of various sizes.

It is, perhaps, unnecessary to point out, that a wrench on this principle may be varied in its form and size so as to be rendered probably more convenient for some particular purposes for which such instruments are required.

I am, Sir,

Your obedient servant,

Portsmouth Dock Yard,

WM. BARLOW.

March 1, 1808.

The instru-
ment de-
scribed.

This instrument is represented in Pl. II. Fig. 5 is a perspective view of it; fig. 6 a section of its head; and fig. 7 an external representation of the head. The screw head or nut to be turned is held between two jaws, one of which *a b d e* is forged in the same piece with the handle *A A*, the other, *f g*, is moveable between two chukes, and fastened to the fixed jaw by the strong screw *i*, which is fixed to the same jaw, passes through the moveable one, as shown in the section fig. 6, and has a nut screwed upon it; the other screw *h*, is tapped through the movable jaw, and its point presses upon the bottom of a cavity made in the fixed jaw shown at *m* in the section fig. 6. To make the wrench fit any particular screw head or nut, the nut upon the strong screw *i* must first be loosened, and the screw *h* screwed in or out of the movable jaw, until the opening *b g* is just the proper width to receive the screw head or nut to be turned by the wrench; the nut of the screw *i* is then to be screwed down, until it presses upon the jaw, and holds it perfectly tight.

Fig 1.

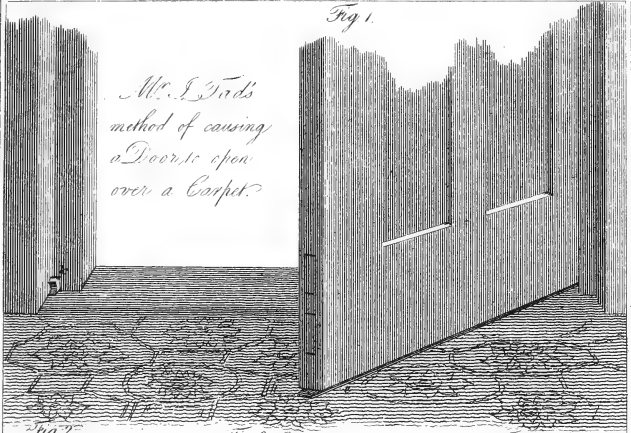


Fig 2.



Fig 3.

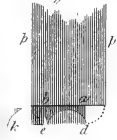
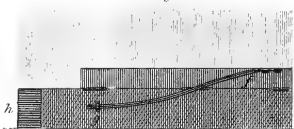


Fig 4.



Mr. W. Barlow's Wrenches for Screw nuts of any size.



Fig 5.

Section

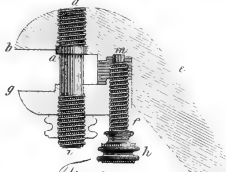


Fig 6.

Enlarged view of the head.

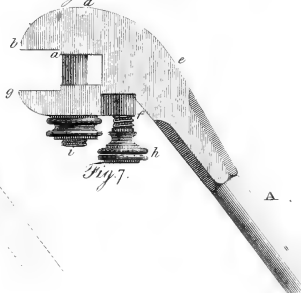


Fig 7.



XIII.

On the Measurement of Heights by the Barometer. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

July 17th, 1809.

THE method of finding heights by the barometer bids fair to be of the greatest practical utility; especially since the improved construction of portable barometers, and the invention of more compendious modes of calculation than those formerly in use, have considerably diminished the difficulties, with which it was at first attended, Finding heights by the barometer of great practical utility.

It would be desirable, however, if the necessary calculations could be still farther simplified: for it must frequently happen, that observations of the heights of the barometer are made by travellers at times, when the mind, distracted by a variety of objects, or borne down by the fatigue of the body, may be ill calculated for even a moderate degree of exertion. Desirable to have the calculations still farther simplified.

For this purpose the following tables have been calculated, which, with little more than the mere trouble of inspection, will give the result true to the nearest foot. They may be printed on the surface of a common card, so that their bulk cannot be the least inconvenience to a traveller.

Table 1. Contains altitudes in feet answering to every tenth of an inch of the height of the barometer from 25 to 31 inches. Use of the following tables.

Table 2. Contains the proportional parts to be deducted for every additional hundredth of an inch, corresponding to the heights of the barometer marked in the first column.

Hence to find the approximate elevation of one station above another, nothing more is necessary, than to find from Tables 1 and 2 the elevations corresponding to the observed heights of the barometer, and subtract the less from the greater.

It would not be difficult to construct tables, which should give

give the result by mere inspection: but unless they should be continued to every hundredth of an inch (in which case they would make a volume) the trouble of subtraction is all that would be saved.

Table 3. Gives the correction for the expansion of air for every 1000 feet of altitude. It is calculated for every degree of the mean temperature from 72 to 32. It is probable that few observations will be made in this island, where the mean temperature is not within these limits.

This table is calculated from Table 5, p. 484, of Gregory's *Mechanics*, vol. 1.

With regard to the correction for the expansion of the mercury, it may be obtained without any sensible error, by multiplying the difference of temperature in degrees of Fahrenheit by 2.75 feet, or 2 feet 9 inches.

Yours, &c.

J. B.

TABLE I.

Bar.	F.	B.	F.	B.	F.
25.0	5606	27.0	3600	29.0	1738
.1	5502	.1	3504	.1	1648
.2	5398	.2	3408	.2	1559
.3	5295	.3	3313	.3	1470
.4	5192	.4	3217	.4	1381
.5	5090	.5	3122	.5	1293
.6	4988	.6	3028	.6	1205
.7	4886	.7	2934	.7	1117
.8	4785	.8	2840	.8	1029
.9	4684	.9	2746	.9	942
26.0	4584	28.0	2653	30.0	855
.1	4483	.1	2559	.1	768
.2	4384	.2	2467	.2	681
.3	4285	.3	2375	.3	595
.4	4186	.4	2283	.4	509
.5	4087	.5	2191	.5	424
.6	3989	.6	2100	.6	338
.7	3891	.7	2009	.7	253
.8	3794	.8	1918	.8	169
.9	3697	28.9	1828	.9	84

TABLE II.

B.	B.
25.0	10.4
.2	—3
.5	—2
.7	—1
26.0	—0
.3	-9.9
.5	—8
.8	—7
27.1	—6
.3	—5
.6	—4
.9	—3
28.2	—2
.5	—1
.9	—0
29.2	-8.9
.5	—8
.9	—7
30.2	—6
.6	—5
31.0	—4

TABLE

TABLE III.

Th.	Cor.	Th.	Cor.	Th.	Cor.
82	126	59	68	45	32
72	101	53	65	44	29
71	98	57	62	43	27
70	96	56	60	42	24
69	93	55	57	41	22
68	91	54	54	40	19
67	88	53	52	39	17
66	86	52	49	38	14
65	83	51	47	37	12
64	80	50	44	36	10
63	78	49	42	35	7
62	75	48	39	34	5
61	73	47	37	33	2
60	70	46	34	32	—

XIV.

On the Glauberite. By ALEXANDER BROGNIART*.

THE form of the glauberite is that of an oblique prism, Figure of the greatly depressed, and with a rhombic base. The angles of the parallelogram of the base are 76° and 104° . The angles of incidence between the parallelogram of the base and the adjacent sides are 142° . That between the base and the edge contiguous to the acute angle of the base is 154° . The faces of the base are generally plane, smooth, and even shining: those of the sides on the contrary are full of striæ, parallel to the edges of the base. Very evident junctures parallel to the base are discoverable by cleaving; as are others not so well defined, parallel to the edges of the base, and inclined to the former in angles of about 104° .

These observations give as the primitive form of this crystal an oblique prism with a rhombic base. Primitive form.

The crystals are nearly limpid, or of a topaz yellow, and retain their solidity and transparency in the air, if they have not been wetted. Colour.

* Journal de Physique, vol. LXVI, p. 295.

- Hardness.** Their hardness exceeds that of sulphate of lime, but is inferior to that of carbonate of lime.
- Action of fire.** Exposed to the fire, the glauberite splits, decrepitates, and melts into a white enamel.
- Singular action of water.** Immersed in water, its surface becomes of a milky white, and in a little time the whole of the crystal grows completely white and opaque. Taken out of the water and dried, it does not resume its transparency, but the white coating falls to powder; and, if it be entirely removed, the nucleus is discovered remaining unaltered. It is the only mineral substance that possesses this property.
- Spec. grav.** The specific gravity of the glauberite is 2.73.
- Crystals resembling it.** This salt, the crystals of which at first sight bear some resemblance to those of axinite, and the fragments of which are a little like those of sulphate of lime, differs essentially from the latter, whether anhydrous or possessing its water of crystallization, in its primitive form, and in the secondary forms derived from it.
- Its component parts.** It is composed of anhydrous sulphate of lime 49
anhydrous sulphate of soda 51
-
- 100
- No water.** Mr. Brongniart satisfied himself, that it contained no water, not only by several calcinations at the temperature nearly of melting silver, but also by distilling it after Mr. Berthollet's manner with iron filings, when he could obtain no hydrogen gas.
- Sulphate of soda,** He ascertained the presence of the sulphate of soda by solution and crystallization, which afforded him well defined crystals of this sulphate.
- and of lime.** The sulphate of lime he found by decomposing this salt both by carbonate of ammonia and oxalate of ammonia.
- No loss.** As he had no loss, but what cannot be avoided in chemical operations conducted with the greatest care, and this loss did not amount to one per cent, he presumes, that this stone contains no other ponderable matter essential to it but the two salts mentioned above: and to be more certain of this, he examined carefully, whether it contained no phosphates,

phosphates, borates, or muriates, which might have been suspected from the situation where it was found.

The glauberite was brought from Spain by Mr. Du-meril. It has hitherto been found only at Villarubia, near Ocanna, in new Castile. Its crystals are sometimes solitary, sometimes in clusters, and disseminated in masses of sal gem. Mr. Brongniart has not been able to find any mention of this mineral; either in the works of mineralogists, or in those of travellers in Spain, that he could consult.

XV.

An excellent colourless Copal Varnish. By Mr. LENORMAND, late Professor of Natural Philosophy.*

EVERY one knows the difficulty of dissolving copal completely, when we attempt to make a varnish, I hasten therefore to communicate a method, that has succeeded perfectly with me; and which will be found, to produce a very fine varnish with this substance.

Copal difficult of solution.

All copal is not fit for making this varnish, it must therefore be selected with care, and the following method will show what is good. Take each piece of copal separately, and let fall on it a single drop of very pure essential oil of rosemary, not altered by keeping. Those pieces on which the oil makes a certain impression, that is to say, which soften at the part that imbibes the oil, are good, and should be reserved for making varnish. The others are to be rejected.

Method of selecting it,

Powder the pieces of copal thus selected, sift the powder through a very fine hair sieve, and put it into a glass, on the bottom of which it must not lie more than a finger's breadth thick. On it pour essence of rosemary to a similar height, stir the whole together with a stick for a few minutes, the copal will dissolve into a viscous substance, and the whole will form a very thick fluid. Let it stand for a couple of hours, after which pour on gently two or three drops of

and of making the varnish.

* Sonpini's *Bibliothèque Physico-économique* for 1808, Vol. II, p. 133.

very pure alcohol, which you will distribute over the oily mass by inclining the glass in different directions with a very gentle motion. In this way you will effect their incorporation. Repeat this operation by little and little, till the varnish is reduced to a proper degree of fluidity. Remember, the first drops of alcohol are the most difficult, and require the longest time to incorporate; and that the difficulty diminishes as each successive addition is incorporated, or as the mass approaches the state of saturation.

When the varnish has attained the suitable degree of fluidity, it is to be suffered to stand a few days; and when it has become very clear, the varnish is to be decanted off.

The magma that remains at the bottom may still be rendered useful, by pouring on alcohol in the manner directed above; but care must be taken, to add very little at a time.

Uses of the
varnish.

This varnish is made without heat, is very clear and colourless, may be applied with equal success on pasteboard, wood, and metals, and may be worked and polished with ease, indeed better than any known varnish. It may be used on paintings, and singularly heightens their beauty.

SCIENTIFIC NEWS.

French National Institute.

French national
Institute.
Question for a
double prize.

THE public sitting of the Mathematical and Physical Class for 1809 was held on the 2nd of January. A double prize had been offered for a "Theory of the perturbations of the planet Pallas, discovered by Dr. Olbers: or in general the theory of planets, the excentricity and inclination of which are too considerable for their perturbations to be calculated with sufficient precision by the known methods." Not to enter into any thing more than is indispensable, on such a difficult subject, nothing was required farther than algebraic formulæ, but so arranged, that an intelligent calculator might apply them securely, and without mistake, either to the planet Pallas, or to any other already discovered, or that may be discovered hereafter. Notwithstanding these restrictions, no paper having been sent, the subject is still

still left open till the 1st of October, 1810. The prize is a medal of the value of 6000 francs [£250].

The ordinary prize subject for next year is: "To examine whether there be any circulation in the animals known by the names of *asteriæ*, *echini*, and *holothuriæ*; and, if there be, to describe its course and organs." The description must be accompanied with observations made on living animals, and include the vessels of the respiratory organs, if there be any such, as well as those of the principal circulation. To examine the chemical effect of the respiration on the air and water, would be a desirable addition, but this is not absolutely insisted on. The examination of one species of each family only is required; but it is expected to be by no means superficial, and accompanied with drawings, so that the principal details may easily be verified. The prize is 3000 francs [£125], and the term as above.

The history of the mathematical division of the class of physical and mathematical sciences exhibits this year a singular circumstance; one of the most difficult and most important points of the solar system treated with equal success, though after different methods, by two geometers of the first rank; to both of whom the investigation was suggested by an interesting paper read to the class by a young geometer. Astronomers had remarked a perceptible acceleration in the course of the moon: consequently other planets, and among them the Earth, must have a similar acceleration. If the motion of the Earth be accelerated, it must be owing to its approaching the centre of motion: and, if it do, will it not ultimately fall into the sun? The danger of this indeed must be infinitely remote, for the acceleration is extremely slow; and it appears from the instance of the moon, that the acceleration continues but for a time, and is afterward changed into retardation. Still however the question is particularly interesting to astronomers, who in all their calculations suppose the unchangeableness of the ellipses described by the planets.

Mr. Laplace first examined this question, and found by a learned but merely approximate calculation, that the mean motions and axes are really invariable; at least taking into consideration only the first powers of the masses, and the second

Another prize question.

Mathematical class.

Problem whether the planets have a constant acceleration.

Laplace showed they have not by an approximate calculation;

which Lagrange extended;

and Poisson has carried still farther.

History of the sciences.

Discoveries in chemistry.

Minerals exposed to great heat under pressure

contain crystals not fused.

Lamination of zinc,

and its extraction from the ore.

Acetic acid from wood.

second of the eccentricities and inclinations. Mr. Lagrange, struck with this conclusion, endeavoured to extend it; and proved by a curious theorem, that the proposition was true, considering even all the successive powers of the eccentricities. But what would be the result, were the masses considered in terms of two dimensions? This inquiry demanded great labour, and no less acumen: yet Mr. Poisson undertook it, and demonstrated, that, if any acceleration exist, it can only depend on terms of four, six, or eight dimensions, and of course must be altogether imperceptible. As soon as Mr. Poisson had demonstrated his theorem, Mr. Lagrange and Mr. Laplace perceived, that it naturally flowed from principles and methods they had formerly laid down: in consequence they were both led to demonstrate the proposition more generally, but each in a different way.

The physical division of the class presented to the emperor a sketch of the history of the sciences from the year 1789, which will soon be published.

The principal discoveries in chemical science are those to which Mr. Davy led the way, and which have been pursued in France chiefly by Messrs. Gay-Lussac and Thenard.

The experiments of Sir James Hall too have been repeated by Mr. Dree. Having exposed to fusion in close vessels, under irresistible pressure, fragments of rocks with trap or chert for their base, he found, that they assumed all the appearance of stony lavas; and that the crystals of feldspar in them were not altered, which explains the singular fact of so many very fusible crystals contained in lavas, that have rendered it questionable whether these lavas had ever been in a state of fusion.

The invention of the art of laminating zinc by heating it is claimed for the late Macquer and Mr. Sage, who practised it long ago: and Messrs. Dony and Poncelet, of the department of the Ourthe, have converted calamine simply by subliming it into metal sufficiently pure to be laminable. The ore affords them one third its weight of metal, which is much cheaper than lead.

Another successful application of chemistry to the arts is that of procuring from wood an acetic acid as pure as radical vinegar, the manufacture of which has been carried on

some

some time by Mr. Mollerat. It answers extremely well for aromatic vinegar; but possesses a little acrimony, on account of which it is not quite so fit for the table. The wood distilled for this purpose yields as much charcoal as in the ordinary way, and a great deal of tar.

In consequence of the interruption between France and the West Indies Mr. Proust and Mr. Parmentier have taken great pains to improve the extraction of sugar from grapes*. Grape sugar.

Mr. Morveau has given a history of attempts to construct instruments to measure high degrees of heat, in which he does Wedgwood more justice, than he has generally received in France. He afterward describes an instrument of his own invention sufficiently delicate to indicate changes in a metallic bar that do not exceed a thirteenth thousandth part of its length. Such a bar of platina is the only thing sufficiently dilatable, and at the same time unalterable by fire, to serve properly for a pyrometer; but the difficulty is to place it on a scale, that will not dilate. This Mr. de Morveau hopes soon to accomplish. Pyrometer.

Mr. Gay-Lussac has just explored a beautiful law of general chemistry on the proportion of metal, that enters into each metallic salt, and that of oxygen necessary for its oxidation. He has shown, that a metal, which precipitates another from an acid solution, finds in the metal precipitated all the oxygen necessary for it to become oxidized, and dissolve in such a quantity, that the solution shall be neutralized to the same degree. The quantity of oxygen remains constant, whatever be the proportion necessary to each metal: and the acid in each salt is proportionate to the oxygen of the oxide, and requires so much more metal to saturate it, in proportion as the metal requires less oxygen for its oxidation. This law affords a very simple method of determining the composition of all metallic salts; for it is sufficient to know the proportion of acid in one salt of each genus, to be acquainted with all; and a single analysis will allow us to dispense with the rest. Law respecting the proportion of metal and oxygen in metallic salts.

Mr. Darcet jun. has shown, that soda and potash, prepared with alcohol and heated to the point at which they Soda and potash cannot be freed from water.

* See Journal, vol. XXI, p. 306, and 341.

begin

begin to evaporate, notwithstanding still retain nearly a third of their weight of water.

Animal mucus and urée.

Messrs. Fourcroy and Vauquelin have presented two important memoirs, one on animal mucus, the other on urée.

Structure of the brain and nervous system.

Among the anatomical subjects, that have engaged the attention of the class, few are so interesting as the memoir on the structure of the brain and nervous system by Drs. Gall and Spurzheim of Vienna. According to these gentlemen, the cinereous or cortical substance is the organ, from which issue the nervous filaments, that constitute the white medullary substance. Wherever the cinereous substance exists, some of these filaments originate; and wherever any of these filaments commence, this substance will be found. The spinal marrow is not a bundle of nerves descending from the brain: on the contrary the nerves termed cerebral may be traced to the medulla oblongata or spinalis; and the brain and cerebellum themselves are but developements of fasciculi from the medulla oblongata, in the same manner as the nerves come from it. The committee have found almost all the anatomical observations of Drs. G. and S. agreeable to nature: but they think it proper to add, that this has no connection whatever with Dr. Gall's theory of the appropriation of different part of the brain to the different functions of the mind.

Analogy of structure in animals.

Prof. Duméril has considered in new points of view the bones and muscles of the trunk in man and various animals. The grand principle he seeks to establish is, that nature is as uniform as possible in her means, continuing the same through numerous varieties, as long as they are effective, and never adding a new organ, unless when new circumstances require greater efforts and more powerful means.

Coats of the nerves composed of nervous filaments.

Mr. Villars of Strasburg, has presented two papers on the structure of the nerves. He thinks he has perceived, by means of the microscope, that the covering of the nerves is itself composed of nervous filaments: but the committee, notwithstanding they have taken great pains to ascertain this, could not satisfy themselves of the fact.

Mirbel,

The anatomy of plants is indebted for many new and important facts to the researches of Mr. Mirbel. The Royal Society of Gottingen, having made this anatomy

a sub-

a subject of one of its annual prizes, has occasioned the publication of several tracts, the principal of which are those of Link, Treviranus, and Rudolph, all professors in different German universities, Agreeing in most facts with Mirbel, they not only add some observations to his, but contradict him on certain points; which has induced him to publish a defence of his theory, in which he gives it more precision, exhibiting it in the form of aphorisms; while he endeavours to show, that most of the objections arise from his having been misunderstood, or his observations not having been repeated with sufficient care.

contradicted in some points,

has defended his theory.

Mr. Mirbel has likewise presented to the class two papers, one on the germination of the family of grasses, the other on the distinguishing characteristics of the monocotyledonous and dicotyledonous plants.

In the first he shows, that the stigmata of wheat unite in a small canal, which reaches to the base of the embryo; and that the cotyledon, as Jussieu thought, is a fleshy substance, in which the radicle and plumula are imperceptibly developed, and which opens lengthways to let them pass, so that it performs the office of a vaginating leaf.

Germination of grasses.

From the second it appears, that the cotyledons have great analogy to the leaves, those of the sensitive plant, being irritable, of the borages hairy, &c.; in short, they are true *leaves in the seed*. If, when there are two cotyledons, they appear opposite in plants the leaves of which are alternate, it is because the stalk cannot develop itself in the seed, and the interval between the cotyledons is not to be distinguished. From the different perceptible analogies between them, Mr. M. infers, that the number of the cotyledons must refer to some circumstance respecting the leaves; and he imagines, that the monocotyledonous plants are uniformly those, the leaves of which ensheath each other. Proceeding to examine the formation of the wood, Mr. M. shows, that it is always composed of filaments interspersed in a cellular texture resembling the medulla of the dicotyledons; but that in many of the monocotyledons these filaments are formed at the circumference as well as in the centre: the latter in consequence having a double vegetation; one at the circumference, increasing the diameter

Cotyledons bear great analogy to leaves.

Monocotyledonous plants.

Wood.

meter

meter of the trunk ; the other at the centre augmenting its density. He considers each of the filaments of the trunk of the monocotyledons as if it answered to an entire trunk of a dicotyledon ; and shows, that in each of these filaments a series of operations takes place as complete as in those trunks.

Mirbel elected to the Institute. Mr. Mirbel, in consequence of his various labours toward illustrating the physiology of plants, was elected to the place vacant by the death of Mr. Ventenat.

Decandolle his competitor. The competitor of Mr. Mirbel for the vacancy in the Institute was Mr. Decandolle, who, beside his previous titles to it, had sent the class early in the year a work on plants with compound flowers, in which he makes a separate family of those the florets of which have two unequal lips, and distributes those termed cinerocephalous according to the lateral or terminal insertion of the seed. It was thought however, that his talents would be more useful in the celebrated school, in which he teaches botany, and at the head of the fine garden under his care, in a climate more favourable to the vegetation of foreign plants than the vicinity of Paris.

Botany much cultivated in France. This sitting showed in general, that botany is cultivated in France with more ardour than ever. The Memoir on the Family of Orchidæ, by Mr. du Petit Thouars, a specimen of a greater work on the natural families of plants, with those of Mr. de Longchamp on Narcissusses, Mr. Jaume St. Hilaire on the Orobanches, and Mr. de Cubières on the Lote trees, and the Monography of Eringums by Mr. de la Roche, are proofs of this.

Developement of the bud. Mr. du Petit Thouars in particular has determined to publish his Theory of Vegetation, founded on the developement of the bud in two directions, which was noticed in our former report, vol. XXIII, p. 315.

New family of plants. Mr. Ventenat himself terminated his laborious career by a paper on the Genera Samyda and Casearia, of which he makes a new family next to that of the rhamnoides. This

Jardin de Cels. piece was intended for the continuation of the *Jardin de Cels*, a work interrupted by his death. He lived long enough to carry to some extent, though not to finish, his

Garden of Malmaison. Description of the Garden of Malmaison, which no doubt will be continued by some other hand. The

The history of animals has witnessed the completion of Mr. Olivier's grand work on coleopterous insects, and is enriched with a description of all the gelatinous animals included under the name of medusa by Linnæus. Mr. Péron, who collected a great number in his voyage to the south, has increased this family to more than a hundred and fifty species. The following is his account of their singularities. " Their substance seems to be merely a coagulated water, yet the most important functions of life are exercised in it. Their multiplication is prodigious, yet we know nothing of the peculiar mode in which it is effected. They are capable of attaining several feet in diameter, and fifty or sixty pounds in weight, yet their nutritive system escapes our eyes. They execute the most rapid and long continued movements, yet the details of their muscular system are imperceptible. They have a very active species of respiration, the true seat of which is a mystery. They appear extremely feeble, yet fish of considerable size form their daily prey, and dissolve in a few moments in their stomach. Many species of them shine amid the darkness of night like balls of fire; and some sting or benumb the hand that touches them: yet the principles and agents of both these properties remain to be discovered."

All the medusas have a gelatinous body, nearly resembling the cap of a mushroom, which Mr. P., after the example of Spallanzani, names *umbrella*; but they differ in wanting or having a mouth; in the mouth being simple or multiplicitous; in the presence or absence of a production resembling a pedicle; and in the edges of this pedicle, or of the mouth itself, being furnished with tentacula, or filaments more or less numerous. From these characters Mr. P. forms divisions and subdivisions, under which every possible kind of medusa may be arranged. Very fine paintings by Mr. Lesueur, who accompanied him on the voyage, illustrate the various forms and colours of these animals, many of which are very pleasing to the eye.

To this examination of their external characters, Mr. P. has added very interesting remarks on the interior structure of these animals; and in particular of that genus, which Mr. Cuvier named *rhizostome*, because he supposed, that the

Olivier's Cole-
opteræ finish-
ed.

Medusæ.

Specific cha-
racters.

Their interior
structure.

the filaments bordering its tentacula were so many suckers; and that the nourishment drawn in by them was received into a central cavity, whence it was distributed to the whole body by an infinite number of vessels disposed with great regularity, and particularly numerous about the edges of the umbrella. The four apertures at the sides of the base of the pedicle appeared to Mr. Cuvier to be the organs of respiration. Mr. P. on the contrary, having seen many living rhizostomes take in small animals by these four apertures, and digest them in the four cavities to which they lead, presumes that they are four mouths, and as many stomachs; while the great vascular apparatus, that fills the pedicle and the borders of the umbrella, is more probably appropriated to respiration, as it is almost always found full of air.

Skeletons of
animals found
in the earth.

Mr. Cuvier read a paper on certain reptiles, the skeletons of which are found in strata of our globe. These had all been taken for crocodiles, and even for the species common in the Ganges, the *gavial*; but the lacerta monitor is also among them, and those that most resemble the *gavial* have striking characteristics to distinguish them. All of them are found in strata much deeper, and consequently more ancient, than those that contain bones of land quad-

Bones of a large
monitor lizard,
26 feet long.

rupeds. The environs of Maestricht conceal the bones of a large animal of this family, which some have taken for a crocodile, others for a fish. Mr. C. attempted to show, that this also was a lacerta monitor, but it is the giant of its kind. It measures in length upward of eight metres [26 feet]. Its tail, much shorter in proportion, but broader, than that of other species, formed a powerful oar; and every thing renders it probable, that it had sufficient strength, and was so good a swimmer, as to live amid the waves of the ocean. Its bones too are found with those of large sea turtle, and among thousands of sea shells.

An inhabitant
of the sea.

Fossile bones
from America.

Mr. Jefferson, President of the United States, has sent the class a fine collection of fossil bones dug up on the banks of the Ohio. The greater number belong to the large animal improperly called mammoth by the Americans, and to which Mr. Cuvier has given the name of mastodonte: but there are likewise some belonging to the true mammoth

of

of the Russians, or the other large animal, much resembling the Indian elephant, the remains of which are so common in Siberia. These two gigantic creatures therefore formerly inhabited together all the northern cap of our globe. The destruction of these enormous races, and of so many others, victims of the same catastrophe, cannot be explained, till we are well acquainted with the strata in which they are buried, as well as their nature and succession.

Mr. Cuvier and Mr. Brongniart have endeavoured to study these in the neighbourhood of Paris. As far as they have been able to penetrate into the earth round that capital, they have found it composed of various strata evidently of different origin. The lowest part is a vast mass of chalk, that reaches to England, and contains nothing but unknown shells, several of which belong even to unknown genera. On this chalk rests a bed of potter's clay, containing no organized body. This in several places is covered by limestone, the hardest of which is used for building, and which is full of shells, most of them of unknown species, but of known genera, or approaching nearer than the preceding to those that live in our present seas. Hills of gypsum are scattered as if by accident sometimes on the clay, at others on the limestone, and contain thousands of bones of land animals entirely unknown, of which Mr. Cuvier has put together the skeletons, and established the characters. In this gypsum, and the clay intermixed with it, or immediately covering it, there are no shells but fresh water ones: but these are afterward covered with thick strata of sea shells. A vast bed of sand, without any organized bodies, crowns all our heights; and, what is most remarkable of all, the most superficial stratum, that which covers the whole, is mixed with fresh water shells alone. It is only in the bottoms of valleys, or in cavities hollowed out of this superficial stratum, that are found the bones of elephants and other animals, the *genus* of which is known, but not the species.

Strata in the vicinity of Paris.

From the observations of these gentlemen it appears, that the sea, having long covered this country, and several times changed its nature and inhabitants, gave place to fresh

The land there long covered by the sea, afterward with fresh

fresh water,
and once or
twice again by
the sea.

fresh water, in which these gypsums were deposited; but that it returned at least once to cover the land it had abandoned, and destroy the beings that had lived on it. On this occasion perished the palæotheria and the anoplotheria. Every thing renders it probable however, that it returned a second time, and that the elephants disappeared in this second catastrophe.

Petrification.

Mr. Sage presented to the class a ferruginous petrification, having some appearance of a bundle of tobacco leaves tied round with threads, but probably part of a stalk of bamboo, or some other jointed plant. He likewise gave descriptions and analyses of a few stones; and communicated some experiments on the cohesion lime contracts with various substances.

Transition strata.

Mr. Brochant, mine engineer, presented some observations on strata much more ancient than those in the vicinity of Paris, which Werner has called transition strata, because they are placed between the primitive mountains, anterior to all organization, and the secondary strata, that abound with remains of animals. Most of them are composed of fragments of the primitive rocks, united into breccias by cements of various kinds, in which we begin to perceive occasionally remains of organized substances, either vegetable or animal. Saussure had already noticed these in the Alps, but Mr. B. has traced them to much greater extent, principally along that side of the Alps which looks toward France.

Alps.

Climate of Genoa.

Mr. Lescallier has shown, that the climate of Liguria is more favourable to the plants of hot countries, than any other in the same latitude: the winter, though longer, not being so cold, because the Apennines shelter it from the north wind; while the summer is less scorching, from the vicinity of the sea on one hand, and the snows on the other.

Department of the Doubs.

Mr. Girod-Chantrans has given the natural history of the department of the Doubs.

Albumen a remedy against intermittents.

Mr. Seguin, who formerly found gelatine the true remedy against intermittent fevers*, has this year tried albumen with good success. He has already cured forty-one patients, by giving them the whites of three eggs diluted with warm

* See Journal, vol. VI, p. 138, and XIII, p. 205.

water,

water, and sweetened with sugar, just before the fit comes on. He says this convenience attends both these remedies, if the fit that follows the first dose be not mitigated, you must not expect a cure from them; if it be, perseverance in them will succeed†.

Messrs. Cels, Tessier, and Huzard, have drawn up a Code of rural laws. scheme for a Code of Rural Law, the object of which is to protect landed property from every imaginable injury. It is transmitted to a select committee in every department for examination.

Mr. Tessier has drawn up, by order of government, popular instructions for the cultivation of cotton in France. Cultivation of cotton in France.

Mr. Bosc has described twenty-eight species of the ash, half of which, though cultivated in the gardens and nurseries round Paris, have not been noticed by naturalists. Superior species of ash. Some of them are large trees, superior in elasticity and flexibility to the common ash.

St. Thomas's and Guy's Hospitals.

The Winter Courses of Lectures at these adjoining Hospitals will commence as usual, the beginning of October. Lectures

Viz. *At St. Thomas's.*

Anatomy and the Operations of Surgery. By Mr. CLINE at St. Thomas's, and Mr. COOPER.

Principles and Practice of Surgery. By Mr. COOPER.

At Guy's.

Practice of Medicine. By Dr. BABINGTON and Dr. Guy's, CURRY.

Chemistry. By Dr. BABINGTON, Dr. MARCET, and Mr. ALLEN.

Experimental Philosophy. By Mr. ALLEN.

Theory of Medicine, and Materia Medica. By Dr. CURRY and Dr. CHOLMELEY.

Midwifery, and Diseases of Women and Children. By Dr. HAIGHTON.

Physiology, or Laws of the Animal Economy. By Dr. HAIGHTON.

Structure and Diseases of the Teeth. By Mr. Fox.

N.B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a *Complete Course of Medical and Chirurgical Instructions*. Terms and other particulars may be learnt at the respective Hospitals.

London Hospital.

Dr. BUXTON's Autumnal course of Lectures on the Theory and Practice of Medicine will commence on the 2d of October at the Medical Theatre. and the London Hospital.

† Have we not here a clew to the presumed success of such apparently inert remedies? C.

METEOROLOGICAL JOURNAL,

For AUGUST, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker,
in the STRAND, LONDON.

JULY Day of	THERMOMETER.				BAROME- TER, 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.		Day.	Night.
26	62	64	70	61	29.75	Rain	Cloudy*
27	63	66	69	61	29.73	Fair	Ditto
28	62	59	65	55	29.64	Rain	Fair
29	59	58	64	58	29.73	Ditto	Rain
30	60	59	64	56	29.60	Ditto	Fair
31	58	58	66	57	29.55	Fair	Ditto
AUG.							
1	61	62	66	57	29.66	Ditto	Cloudy
2	60	62	66	60	29.72	Ditto	Ditto
3	60	57	62	51	29.40	Rain	Fair
4	53	55	59	51	29.41	Ditto †	Cloudy
5	53	58	61	57	29.76	Ditto	Rain
6	61	59	63	55	29.38	Ditto	Ditto
7	58	59	64	56	29.65	Ditto	Fair
8	63	64	68	58	29.94	Fair	Ditto
9	64	65	71	58	29.94	Ditto	Ditto
10	62	70	74	60	29.89	Ditto	Cloudy ‡
11	61	61	73	60	29.68	Ditto	Fair
12	63	57	68	59	29.67	Rain	Ditto
13	64	63	68	60	29.79	Ditto	Ditto
14	64	63	69	58	29.80	Ditto	Ditto
15	61	61	63	58	29.75	Ditto	Cloudy
16	61	63	67	60	29.88	Ditto	Fair
17	63	64	76	58	29.84	Fair §	Ditto
18	60	61	71	59	29.78	Rain ¶	Ditto
19	63	61	68	57	29.80	Ditto	Ditto
20	61	61	66	56	29.96	Ditto	Ditto
21	60	58	64	52	29.84	Ditto	Ditto
22	58	57	67	52	29.84	Ditto	Ditto
23	59	57	67	50	29.62	Fair	Ditto
24	58	56	64	48	29.49	Rain	Ditto
25	55	50	63	48	29.43	Ditto *	Ditto

* Thunder, lightning, and rain in the evening, the moon bright at intervals.

† Rainy and cold, almost the whole day.

‡ Thunder, lightning, and rain in the night.

§ Sultry morning.

|| Lightning in the East, at 11 P.M. very dark and appearance of rain.

¶ Heavy rain in the morning.

* Thunder, at 1 and 3 P.M.

A JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1809.

ARTICLE I.

*Further Application of a Series to the Correction of the Height
of the Barometer.*

To Mr. NICHOLSON.

SIR,

IN calculating a table of the depression of the mercury in the tube of a barometer, produced by the effect of capillary attraction, I have found it necessary to determine a greater number of the coefficients of the series published in your twenty-second volume, p. 213. The value of f is found

$$14b^2 + \frac{21b^7}{5m} + \frac{529b^5}{1920m^2} + \frac{163b^3}{57600m^3} + \frac{b}{3686400m^4}; \text{ and } g,$$

if I have computed correctly, is equal to $42b^{11} + \frac{385b^9}{24m}$

$$+ \frac{129721b^7}{69120m^2} + \frac{983b^5}{23040m^3} + \frac{5197b^3}{33177600m^4} + \frac{b}{530841600m^5}.$$

Here it may be observed, that the numerical coefficients of

VOL. XXIV. No. 107.—OCT. 1809.

G the

Continuation
of a former se-
ries.

the highest powers of b form this series, $\frac{2}{2}, \frac{6}{3}, \frac{6 \times 10}{3 \times 4}, \frac{6 \cdot 10 \cdot 14}{3 \cdot 4 \cdot 5}$, which may be continued at pleasure, and which must obviously express the versed sine of a circular arc, since, when b becomes infinite, the curve coincides with a circle, and the highest powers of b must in this case be infinitely greater than all the rest: the coefficients of the powers of b next in order form this series, $\frac{1}{4 \cdot 4}, \frac{10}{6 \cdot 6}, \frac{10}{8 \cdot 8} \cdot \frac{14}{2}, \frac{10}{10 \cdot 10} \cdot \frac{14}{2} \cdot \frac{18}{3}, \frac{10}{12 \cdot 12} \cdot \frac{14}{2} \cdot \frac{18}{3} \cdot \frac{22}{4}$, and those of the other orders of terms seem to follow a law nearly similar, but which I have not fully ascertained. The coefficients of the terms including the lowest powers of b are however of more consequence; the progression of the coefficients of b is sufficiently obvious: those of b^2 have their denominators increased according to the same law, and the ratio of the numerators approximates to 8, and differs so little from it, that this number may be employed as a multiplier without sensible error: and in a similar manner if the denominators of the coefficients of b^3 are made to increase in the same ratio, the numerators approach nearer and nearer to the ratio of 16 to 1, which is probably their ultimate proportion: and in some instances the continuation of the progressions on these principles is required for a sufficiently accurate determination of the quantities concerned.

Arrangement
of the series.

For calculating the depression or elevation of a fluid in a tube of a given diameter, it is convenient to arrange the series according to the powers of b ; so that the whole assumes this form, $n =$

$$\begin{aligned}
(2 m x & + (.166\ 667\ x^5 \\
+ .25\ x^3 & + .034\ 722\ 2\ \frac{x^7}{m} \\
+ .010\ 4167\ \frac{x^5}{m} & + .003\ 559\ 03\ \frac{x^9}{m^2} \\
+ .000\ 217\ 014\ \frac{x^7}{m^2} & + .000\ 235\ 822\ \frac{x^{11}}{m^3} \\
+ .000\ 002\ 712\ 67\ \frac{x^9}{m^3} & + .000\ 011\ 887\ \frac{x^{13}}{m^4} \\
+ .000\ 006\ 022\ 605\ 6\ \frac{x^{11}}{m^4} & + .000\ 000\ 399\ 6\ \frac{x^{15}}{m^5} \\
+ .000\ 000\ 000\ 134\ 557\ \frac{x^{13}}{m^5} & + .000\ 000\ 011\ 1\ \frac{x^{17}}{m^6} \\
+ .000\ 000\ 000\ 000\ 600\ 8\ \frac{x^{15}}{m^6} & + .000\ 000\ 000\ 247\ \frac{x^{19}}{m^7} \\
+ .000\ 000\ 000\ 000\ 002\ 086\ \frac{x^{17}}{m^7} & + \dots) b^3 \\
+ .000\ 000\ 000\ 000\ 000\ 0058\ \frac{x^{19}}{m^8} & \\
+ \dots) b &
\end{aligned}$$

$$\begin{aligned}
+ (.25\ x^7 & + (.5\ x^9 & + 3\ x^{13}\ b^{11} + \dots \\
+ .109\ 375\ \frac{x^9}{m} & + .35\ \frac{x^{11}}{m} & \\
+ .022\ 960\ 1\ \frac{x^{11}}{m^2} & + .134\ \frac{x^{13}}{m^2} & \\
+ .003\ 047\ 49\ \frac{x^{13}}{m^3} & + \dots) b^7 & \\
+ .000\ 245\ 00? \frac{x^{15}}{m^4} & + (.1\ 167\ x^{11} & \\
+ .000\ 014\ 40? \frac{x^{17}}{m^5} & + 1\ 146\ \frac{x^{13}}{m} & \\
+ .000\ 000\ 69? \frac{x^{19}}{m^7} & + \dots) b^9 & \\
+ \dots) b^5 & &
\end{aligned}$$

The value of b being determined by the solution of this Expansion. equation for any given tube, of which the semidiameter is

x , that of y may be found from the original series, which may be thus expanded,

$$\begin{aligned}
 y = & (4 m \\
 & + x^2 \\
 & + \cdot 0625 \frac{x^4}{m} \\
 & + \cdot 001736 \, 11 \frac{x^6}{m^2} \\
 & + \cdot 000 \, 027 \, 1267 \frac{x^8}{m^3} \\
 & + \cdot 000 \, 000 \, 271 \, 267 \frac{x^{10}}{m^4} \\
 & + \cdot 000 \, 000 \, 001 \, 88380 \frac{x^{12}}{m^5} \\
 & + \cdot 000 \, 000 \, 000 \, 009 \, 611 \, 21 \frac{x^{14}}{m^6} \\
 & + \cdot 000 \, 000 \, 000 \, 000 \, 037 \, 54 \frac{x^{16}}{m^7} \\
 & + \cdot 000 \, 000 \, 000 \, 000 \, 000 \, 115 \, 9 \frac{x^{18}}{m^8} \\
 & + \cdot 000 \, 000 \, 000 \, 000 \, 000 \, 000 \, 29 \frac{x^{20}}{m^9} \\
 & + \cdot 000 \, 000 \, 000 \, 000 \, 000 \, 000 \, 000 \, 6 \frac{x^{22}}{m^{10}} \\
 & + \dots) b
 \end{aligned}$$

$$\begin{aligned}
 & + (x^4 \\
 & + \cdot 277 \, 778 \frac{x^6}{m} \\
 & + \cdot 035 \, 590 \, 3 \frac{x^8}{m^2} \\
 & + \cdot 002 \, 829 \, 86 \frac{x^{10}}{m^3} \\
 & + \cdot 000 \, 156 \, 642 \frac{x^{12}}{m^4} \\
 & + \cdot 000 \, 006 \, 393 \, 4 \frac{x^{14}}{m^5}
 \end{aligned}$$

$$+ \cdot 000$$

$$\begin{aligned}
& + \cdot 000\ 000\ 200 \frac{x^{16}}{m^6} \\
& + \cdot 000\ 000\ 004\ 94 \frac{x^{18}}{m^7} \\
& + \cdot 000\ 000\ 000\ 10 \frac{x^{20}}{m^8} \\
& + \cdot 000\ 000\ 000\ 0016 \frac{x^{22}}{m^9} \\
& + \dots) b^3
\end{aligned}$$

$$\begin{aligned}
& + (2 x^6 & + (5 x^8 \\
& + 1\cdot 09\ 375 \frac{x^8}{m} & + 4\cdot 2 \frac{x^{10}}{m} \\
& + \cdot 275\ 514 \frac{x^{10}}{m^2} & + 1\cdot 87675 \frac{x^{12}}{m^2} \\
& + \cdot 042\ 665 \frac{x^{12}}{m^3} & + \dots) b^7 \\
& + \cdot 003\ 920 ? \frac{x^{14}}{m^4} & + (14 x^{16} \\
& + \cdot 000\ 261 ? \frac{x^{16}}{m^5} & + 16\cdot 04167 \frac{x^{18}}{m} \\
& + \cdot 000\ 0137 ? \frac{x^{18}}{m^6} & + \dots) b^9 \\
& + \cdot 000\ 000\ 55 ? \frac{x^{20}}{m^7} & + 42 x^{12} b^{11} + \dots \\
& + \cdot 000\ 000\ 018\ 2 ? \frac{x^{22}}{m^8} \\
& + \dots) b^5
\end{aligned}$$

In this manner the following table has been calculated, Table of the
 m being still made $\cdot 005$, and $n = \cdot 00375$; and it may in depression of
 general be considered as accurate to the last place of the mercury:
 the depression is also determined according to the experiments of Mr. Gay Lussac, in which
 m appeared to be $\cdot 0051$, n being still $\cdot 00375$. In applying
 the correction to a given barometer, the bore might be as-
 certainied by measuring the difference of the central and
 marginal depressions with a micrometer, and comparing it
 with this table, without the trouble of emptying the tube.

Diameter

Diameter.	Central depression.		Observed by Ld. C. C.	Marginal depression.	Difference.
	$m=.005$	$m=.0051$		$m=.005$	
1.00	.00031	.00032			
.90	.00060	.00062			
.80	.00115	.00118			
.70	.00220	.00224			
.60	.00411	.00416	.005	.0637	.0596
.50	.00799	.00805	.007	.0676	.0596
.45	.01100	.01106		.0690	.0580
.40	.01516	.01522	.015	.0714	.0562
.35	.02093	.02098	.025	.0745	.0536
.30	.02902	.02906	.036	.0787	.0497
.25	.04064	.04067	.050	.0850	.0444
.20	.05800	.05802	.067	.0966	.0386
.15	.08620	.08621	.092	.1171	.0309
.10	.14027	.14027	.140	.1619	.0216
.05	.29497	.29497		.3060	.0110

Elevation of
water.

When it is required to continue the curve till it becomes perpendicular to the absciss, it is evident that the series cannot be sufficiently accurate, since in this case the least imaginable increase of the absciss would afford an impossible value for the ordinate. It is therefore convenient to compute the value of b and y for a portion of the curve a little less than that which is required, and to determine the length of the remainder from its mean curvature, deduced from the magnitude of the ordinate, together with that of the absciss. For example, if it be required to find the central and marginal elevation of the surface of water contained in tubes 1 inch, $\frac{1}{2}$, and $\frac{1}{4}$ of an inch in diameter, taking $m = .01$; we may continue the curve till its inclination to the horizon becomes 60° , and $\frac{n}{m} = .866$; but we must first

determine the corresponding diminution of the diameter, in order to obtain the value of x . For this purpose the part of the curve which is nearly vertical may be compared with a cubical parabola, the distance of which from its tangent is to the versed sine of the osculating circle, as the distance from the vertex, diminished by one third of the tangent, to the whole distance. In the first example, taking the marginal elevation by conjecture .15, we must deduct .02; the height corresponding to the horizontal curvature, of which

the

the radius is $\cdot 5$, and to find the mean radius of the arc of 30° , we have $2s(\cdot 13 - \frac{1}{2}s) = \cdot 01$, $ss - \cdot 26s = -\cdot 01$, $s = \cdot 13 - \sqrt{\cdot 0069} = \cdot 0463$: but the versed sine of the arc of which this is the sine, in the circle of curvature at the vertical point, is $\cdot 01552$, which is to be diminished in the ratio of $\cdot 13 - \cdot 0154$ to $\cdot 13$, and becomes $\cdot 01371$; and deducting this from $\cdot 5$, we have $\cdot 4863$ for the value of x , when $n = \cdot 00866$. Hence we find $b = \cdot 0948$, $a = \cdot 0038$, and the marginal elevation $\cdot 151$, which is so near the assumed value, that no further correction is required. In the same manner, for tubes of $\frac{1}{2}$ and $\frac{1}{4}$ inch in diameter, we find $a = \cdot 0374$, and $\cdot 130$, and the marginal elevation $\cdot 162$ and $\cdot 220$ respectively. But it would be rather more accurate to compute the extent of a portion of the curve, somewhat greater than 60° , by means of the series.

We may also obtain a series, in a manner nearly similar, for determining the relation of the arc to the absciss and the ordinate; and such a series must represent the properties of the curve in a more general manner, and may, in some cases, be more convenient for calculation, at the same time that it affords a mode of verifying the results which we have already obtained. Taking the expression $\int xy \dot{x} \sqrt{(\dot{x}^2 + \dot{y}^2)} = m x \dot{y}$, we may put $\dot{x}^2 + \dot{y}^2 = \dot{z}^2$, $x = z + A z^3 + B z^5 + C z^7 + \dots$, and $y = a + b z^2 + c z^4 + d z^6 + \dots$; then $\frac{\dot{x}^2}{\dot{z}^2} + \frac{\dot{y}^2}{\dot{z}^2} = 1$; but $\frac{\dot{x}^2}{\dot{z}^2} = 1 + 6$

Series in terms of the arc.

$$A z^2 + (10 B + 9 A^2) z^4 + (14 C + 30 A B) z^6 + (18 D + 42 A C + 25 B^2) z^8 + \dots, \text{ and } \frac{\dot{y}^2}{\dot{z}^2} = 4 b^2 z^2 + 16 b$$

$$c z^4 + (24 b d + 16 c^2) z^6 + (32 b e + 48 c d) z^8 + \dots; \text{ whence, by comparing the homologous terms, } A = -\frac{3}{2} b^2,$$

$$B = \frac{-16 b c - 9 A^2}{10}, C = \frac{-24 b d - 16 c c - 30 A B}{14},$$

$$\text{and } D = \frac{-32 b e - 48 c d - 42 A C - 25 B B}{18}. \text{ Again,}$$

$$\text{for the fluent } \int xy \dot{x}, \text{ we have } xy = a z + (a A + b) z^3 + (a B + b A + c) z^5 + \dots, \dot{x} = \dot{z} + 3 A z^2 \dot{z} + 5 B z^4 \dot{z} + \dots, \text{ and } \int xy \dot{x} = a \frac{z^2}{2} + (a A + b + 3 A a)$$

z^4

$$\frac{z^4}{4} + (aB + bA + c + 3A(aA + b) + 5Ba) \frac{z^6}{6} + \dots, \text{ which must be equal to } mx \frac{y}{z}, \text{ or to } 2mbz^3 + (4mc + 2mbA)z^4 + (6md + 4mcA + 2mbB)z^5 + \dots;$$

whence $b = \frac{a}{4m}$, $c = \frac{aA + b + 3Aa - 8mbA}{16m}$ and $d = \frac{aB + bA + c + 3A(aA + b) + 5Ba - 24mcA - 12mbB}{36m}$;

and, by reduction, $c = \frac{b}{16m} - \frac{b^3}{3}$,
 $d = \frac{b}{576m^2} - \frac{4b^3}{45m} + \frac{2b^5}{45}$, $e = \frac{b}{36864m^3} - \frac{302b^3}{26880m^2} + \frac{122b^5}{4032m} - \frac{b^7}{315}$, and $A = -\frac{1}{3}b^2$, $B = -\frac{b^2}{10m} + \frac{2b^4}{15}$,
 $C = -\frac{5b^2}{672m^2} + \frac{2b^4}{35m} - \frac{4b^6}{315}$, and $D = \frac{-7b^2}{20736m^3} + \frac{493b^4}{45360m^2} - \frac{b^6}{70m} + \frac{2b^8}{2835}$.

Here we may observe, that in the series for finding the value of y , the coefficients of the terms involving the lowest powers of b are the same as in the former case, and that there is a similar approximation to the ratios of 8 and 16 in the neighbouring terms, so that we may safely continue the series on these foundations: the coefficients of the highest powers may be found by this

progression, $1, \frac{4}{3 \cdot 4}, \frac{4}{3 \cdot 4} \cdot \frac{4}{5 \cdot 6}, \frac{4}{3 \cdot 4} \cdot \frac{4}{5 \cdot 6} \cdot \frac{4}{7 \cdot 8}$, which,

for the reason already mentioned, must represent the versed sine of a circular arc. In the series for x , the coefficients

of the first terms form this progression; $\frac{2}{3}, \frac{2}{5} \cdot \frac{3}{12}, \frac{2}{7} \cdot \frac{3}{12^2}$

$\cdot \frac{5}{4}, \frac{2}{9} \cdot \frac{3}{12^3} \cdot \frac{5}{4^2} \cdot \frac{7}{5} \cdot 2, \frac{2}{11} \cdot \frac{3}{12^4} \cdot \frac{5}{4^3} \cdot \frac{7}{5^2} \cdot \frac{9}{6} \cdot 2^2 \cdot 3,$

$\frac{2}{13} \cdot \frac{3}{12^5} \cdot \frac{5}{4^4} \cdot \frac{7}{5^2} \cdot \frac{9}{6^2} \cdot \frac{11}{7} \cdot 2^3 \cdot 3^2 \cdot 4$, or $\frac{2}{3} \cdot \frac{1}{1} \cdot \frac{2}{5} \cdot$

$\frac{3}{2 \cdot 2 \cdot 3}, \frac{2}{7} \cdot \frac{3 \cdot 5}{2 \cdot 2 \cdot 3 \cdot 2 \cdot 2 \cdot 3 \cdot 4}, \frac{2}{9} \cdot \frac{3 \cdot 5}{2 \cdot 2 \cdot 3 \cdot 2 \cdot 2 \cdot 3}$

$$\frac{7.2}{.4.2.2.3.4.5}, \frac{2}{11} \cdot \frac{3.5.7.2.2.3}{.2.2.3.2.2.3.4.2.2.3.4.5}$$

$$\frac{.2.2.3.4.5.6}{3} : \text{ and those of the last terms are } \frac{2}{3},$$

$$\frac{2}{3} \cdot \frac{4}{4.5}, \frac{2}{3} \cdot \frac{4}{4.5} \cdot \frac{4}{6.7}, \frac{2}{3} \cdot \frac{4}{4.5} \cdot \frac{4}{6.7} \cdot \frac{4}{8.9}, \text{ or}$$

$$\frac{2}{3}, \frac{2^3}{3..5}, \frac{2^5}{3..7}, \frac{2^7}{3..9} : \text{ and this series must obviously}$$

represent the sine of a circular arc, since all the other terms vanish in comparison with these, when b becomes infinite.

These series however have not the convenience of afford- Inconven-
ing a fluent divisible by x the absciss, as in the former case, ences.
and the expression for the inclination of the curve is much
less convergent: it may however be employed where great
accuracy is not required. Since $\int x y \dot{x} = n x$, we find,

from the first equation, $n x \dot{z} = m x \dot{y}$, and $\frac{\dot{y}}{\dot{z}} = \frac{n}{m}$, con-

sequently $\frac{\dot{x}}{\dot{z}} = \sqrt{1 - \frac{n n}{m m}}$, and the relation of z and

b may be determined from either of the series, when n
and x are given. The series themselves may be thus ex-
panded.

$$\begin{aligned} x=z & + (.133333 z^5 + .007 054 67 z^9 b^8 \text{ Expansion of} \\ & - (.666667 z^3 + .057 1429 \frac{z^7}{m} + \dots \text{the series.} \\ & + .10 \frac{z^5}{m} + .010868 5 \frac{z^9}{m^2} - .000 256 533 \\ & + .00744048 \frac{z^7}{m^2} + \dots) b^4 + .000 006 577 77 \\ & + .000 337 577 \frac{z^9}{m^3} - (.012 698 4 z^7 \\ & + .000 010 357 5 \frac{z^{11}}{m^4} + .014 285 7 \frac{z^9}{m} \\ & + \dots) b^2 + \dots) b^6 \end{aligned}$$

$$y = (4 m$$

$$\begin{aligned}
y &= (4m & - (.333333 z^4 \\
& + z^2 & + .088889 \frac{z^6}{m} \\
& + .0625 \frac{z^4}{m} & + .011235 \frac{z^8}{m^2} \\
& + .00173611 \frac{z^6}{m^2} & + .00089174 \frac{z^{10}}{m^3} \\
& + .0000271267 \frac{z^8}{m^3} & + .00004948 \frac{z^{12}}{m^4} \\
& + .000000271267 \frac{z^{10}}{m^4} & + .00000202 \frac{z^{14}}{m^5} \\
& + \dots \text{as above) } b & + .000000063 \frac{z^{16}}{m^6} \\
& + (.044444 z^6 & + .0000000016 \frac{z^{18}}{m^7} \\
& + .030258 \frac{z^8}{m} & + .000000000031 \frac{z^{20}}{m^8} \\
& + .0084528 \frac{z^{10}}{m^2} & + \dots) b^2 \\
& + .00130? \frac{z^{12}}{m^3} & + (.0031746 z^8 \\
& + .090130? \frac{z^{14}}{m^4} & + .0060317 \frac{z^{10}}{m} \\
& + .000009? \frac{z^{16}}{m^5} & + \dots) b^7 \\
& + \dots) b^5 & + .000042755 z^{12} + \dots) b^{11} \\
& + .0014109 z^{10} + \dots) b^9 & + .0000009344 z^{14} + \dots) b^{13} \\
& + .0000009344 z^{14} + \dots) b^{13} & + .00000001557 z^{16} + \dots) b^{15} \\
& + \dots &
\end{aligned}$$

Other parts of
the curve.

The same mode of investigation may be applied to the more accurate determination of the properties of the curve at any other point of its extent; substituting $r - x$ for x , $q - y$ for y , and $p - f[(r - x) \cdot (q - y) \dot{x}]$ for the fluent: but the calculations become considerably complicated. Thus, if we suppose z to begin where the curve is vertical,

$$\begin{aligned}
\text{we have } x &= bz^2 - \frac{z^3}{6m} - \frac{36mqb^2 - 9r + qr + 144}{576mrb - 72m} \\
&\frac{m^2rb^4 - 144m^2b^3}{z^4} + \dots, \text{ and } y = z - \frac{1}{3}b^2z^3 + \frac{bz^4}{4m} \\
&+ \dots
\end{aligned}$$

+ ... : or, if we express x in the powers of y , $x = by^2 + \frac{2by^3}{3q + 24mb} + \dots$. For example, in the case of water rising in a tube an inch in diameter, q being $\cdot 151$, and $b = \frac{q}{2m} - \frac{1}{2r} = 6\cdot 55$, we have, for an arc of 30° , $\text{Sin. } 30^\circ = \cdot 5 = 13\cdot 1z - 50z^2 - 145\cdot 6z^3$, whence $z = \cdot 05$, or perhaps $\cdot 0505$, $x = \cdot 0161$, and $y = \cdot 0483$. But for this value of y , x ought to be but about $\cdot 0153$: and this difference, as well as the numbers obtained from the properties of the cubic parabola, shows only that it would be better to extend the calculation to an arc of 70° or 80° by the first series, if great accuracy were required. According to Mr. Gay-Lussac's experiments, m is more correctly, in the case of water $\cdot 0115$; in that of pure alcohol $\cdot 0047$.

For a surface of simple curvature, the primary equation Surface of simple curvature. is $\int y \dot{x} \sqrt{(\dot{x}^2 + \dot{y}^2)} = m\dot{y}$, and the coefficients of the first

series become $b = \frac{a}{2m}$, $c = b^3 + \frac{b}{12m}$, and $d = 2b^5 + \frac{11b^3}{30m} + \frac{b}{360m^2}$: the ratios of the last terms being $\frac{1}{5\cdot 6m}$, $\frac{1}{7\cdot 8m}$, and so forth: and $n = \frac{1}{2}bx^3 + \frac{1}{2}cx^5 + \frac{1}{2}dx + \dots$:

and from this series we may calculate the elevation of a fluid between two plane surfaces.

I am, Sir,

Your very obedient servant,

3 Sept. 1809.

E. F. G. H.

II.

On the Action of the Metal of Potash on Metallic Salts and Oxides, and on Alkaline and Earthy Salts. By Messrs. THENARD and GAY-LUSSAC.*

Muriatic acid not obtainable separate. **C**ONVINCED by a number of experiments, that it was not possible to obtain muriatic acid free from every other substance, we attempted to make the metal of potash act directly on muriates, in order to ascertain whether this acid would not by these means undergo some alteration.

Muriate of barytes exposed to the action of potassium. For this purpose we took muriate of barytes fused at a red heat. We had powdered it, and introduced it into a tube of glass blown by the lamp, into which we had previously put a small ball of the metal; but no action took place, either cold or at a red heat; the metal passed through the salt without any perceptible alteration, and on throwing it into water, after the refrigeration of the matter, it inflamed very vividly. Other alkaline muriates did not afford us more satisfactory results. We then subjected to the same trial, in the same way, insoluble metallic muriates, as the muriate of silver, and mild muriate of mercury. Scarcely was the heat greater than sufficed to fuse the metal, when a very vivid inflammation was excited, and these two salts were reduced. In both reductions the tube was broken; and in that of the muriate of mercury, there was something like a slight detonation owing to the mercurial vapour. In both cases nothing was formed but muriate of potash, and no sign of the muriatic acid being decomposed was observed.

No action took place.

Other alkaline muriates.

Muriates of silver and mercury acted upon by potassium,

but the acid not decomposed.

Examination of the action of potassium on other salts and metallic oxides. Having no farther hope of finding a mean of decomposing muriatic acid in experiments of this kind, we attempted to ascertain the action of the metal of potash on other salts, and on the metallic oxides, continuing to employ the same method of operating as before. In all our experiments the heat was constantly a little higher than was necessary to fuse the metal. Sometimes, as far as the decomposition of phos-

* Journal de Physique, January, 1809, p. 103.

phate of lime, sulphate of barytes, oxide of zinc, &c. it was carried to near 300° of the centigrade thermometer [572° F.]. The tubes we used were always broken, when the inflammation was very vivid. To avoid minutiae, we shall confine ourselves to the results we observed.

Sulphate of barytes. Decomposed without any inflammation, and sulphate of barytes obtained. On sulphate of barytes;

Sulphite of barytes. Vivid inflammation, and sulphuret of barytes formed. sulphite of barytes,

From these two experiments we should infer, that the oxygen is much less condensed in the sulphite, than in the sulphate of barytes; and very probably also less condensed in sulphurous acid, than in sulphuric.

Sulphite of lime. Slight inflammation: formation of a very yellow sulphuret. and of lime;

Sulphate of lead. Very vivid inflammation.

sulphate of lead, and of mercury;

Sulphate of mercury but little oxidized. Inflammation as with the mild muriate.

Nitrate of barytes. Very vivid inflammation, and prosection of the matter. nitrate of barytes,

Nitrate of potash. Destruction of the metal without inflammation; which is owing, no doubt, to the nitre containing water. and of potash;

Superoxigenized muriates. Very vivid inflammation. oximuriates;

Phosphate of lime. Decomposition without appearance of inflammation: production of phosphuret of lime. phosphate of lime,

Carbonate of lime. Decomposition without inflammation: charcoal set free. and carbonate;

Chromate of lead. Vivid inflammation.

chromate of lead, and of mercury;

Chromate of mercury. Became slightly redhot: the mass changed green.

Arseniate of cobalt. Vivid inflammation.

arseniate of cobalt; oxides of tungsten, mercury,

Green and yellow tungstic acid. Vivid inflammation.

Red oxide of mercury. Very vivid inflammation: slight detonation owing to mercurial vapour.

Oxide of silver. Very vivid inflammation.

silver,

Brown oxide of lead. Like the preceding.

lead,

Red oxide of lead. Vivid inflammation.

Yellow oxide of lead. The same.

Yellow

copper, Yellow and brown oxides of copper. Vivid inflammation.
 arsenic, White oxide of arsenic. Inflammation.
 cobalt, Black oxide of cobalt. Like the preceding.
 antimony, Volatile oxide of antimony. Inflammation, but less vivid than with the oxides of copper.

Oxide of antimony at a maximum. Very vivid inflammation.

tin, Oxide of tin at a maximum. Very vivid inflammation.
 Putty of tin. Inflammation, but less vivid than the preceding.

iron, Red oxide of iron. Very slight inflammation.
 Black oxide. No inflammation, but reduction of the iron.

manganese, Oxides of manganese at a maximum. Very vivid inflammation.

Oxide at a minimum. No inflammation.

bismuth, Yellow oxide of bismuth. Vivid inflammation.

zinc, White oxide of zinc. Reduction without inflammation.

nickel, Gray oxide of nickel. Pretty vivid inflammation.

and chrome. Green oxide of chrome. No inflammation: production of a blackish matter, which, when completely cooled, and afterward exposed to the air, takes fire as an excellent pyrophorus, and becomes yellow. This matter is a combination of potash and oxide of chrome, which in the air changes to chromate of potash.

Action of potassium on earths. We likewise tried the action of the metal of potash on earths, and particularly on zircon, silex, yttria, and barytes, and found, that it was evidently altered by all these; but as we do not yet well know the cause of this alteration, we shall not here enter into any particulars respecting it. We shall only say, it appears to us very probable, that the phenomena observed in burning the metal of potash in silicious fluoric gas are in no respect owing to the silex.

Siliceous fluoric gas.

Decomposes all substances containing oxygen, Be this as it may, it follows from all the preceding facts, that every substance, in which the presence of oxygen is hitherto known, is decomposed by the metal of potash: that almost all these decompositions take place with extrication of light and heat: that more is disengaged in proportion as the oxygen is less condensed: and that consequently they

afford

afford means of estimating the degree of condensation of oxygen in each substance.

These experiments, having occupied a great deal of time, have prevented us from continuing those we had begun on boracic acid. Yet we had already learned, that this acid is capable of being decomposed at a very high temperature by a mixture of charcoal with iron or platina, and forming borurets: for Mr. Descotils, on exposing such mixtures to a forge fire, has obtained metallic buttons, which, treated with nitromuriatic acid, yielded him very evident quantities of boracic acid; and which, from our experiments on the nature of the boracic acid, could be nothing but a combination of bore, platina, and iron.

Boracic acid decomposable by a mixture of charcoal and metal.

III.

The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, &c. By HUMPHRY DAVY, Esq., Sec. R. S., F. R. S. Ed., and M. R. I. A.

(Concluded from page 24.)

8. Analytical Experiments on Muriatic Acid.

I Have made a greater number of experiments upon this substance, than upon any of the other subjects of research that have been mentioned; it will be impossible to give any more than a general view of them within the limits of the Bakerian lecture.

Numerous experiments made on muriatic acid.

Researches carried on some years ago, and which are detailed in the Journals of the Royal Institution, showed, that there were little hopes of decomposing muriatic acid, in its common form, by Voltaic electricity. When aqueous solution of muriatic acid is acted upon, the water alone is decomposed; and the Voltaic electrization of the gas affords no indications of its decomposition; and merely seems to show, that this elastic fluid contains much more water than has been usually suspected*.

Muriatic acid gas contains much water.

I have already laid before the Society an account of some experiments made on the action of potassium on muriatic

* See p. 31.

acid.

acid. I have since carried on the same processes on a larger scale, but with precisely similar results.

Its action on potassium.

When potassium is introduced into muriatic acid gas, procured from muriate of ammonia and concentrated sulphuric acid, and freed from as much moisture as muriate of lime is capable of attracting from it, it immediately becomes covered with a white crust, it heats spontaneously, and by the assistance of a lamp acquires in some parts the temperature of ignition, but does not inflame. When the potassium and the gas are in proper proportions, they both entirely disappear; a white salt is formed, and a quantity of pure hydrogen gas evolved, which equals about one third of the original volume of the gas.

8 grs. of potassium absorb 22 cub. inches of the gas.

By eight grains of potassium employed in this way, I effected the absorption of nearly twenty-two cubical inches of muriatic acid gas; and the quantity of hydrogen gas produced was equal to more than eight cubical inches.

Hydrogen evolved in the same proportion as if water had been used.

The correspondence between the quantity of hydrogen generated in cases of this kind, and by the action of potassium upon water; combined with the effects of ignited charcoal upon muriatic acid gas, by which a quantity of inflammable gas is produced equal to more than one third of its volume; seemed to show, that the phenomena merely depended upon moisture combined with the muriatic acid gas*.

Farther proof, that nothing but water was decomposed.

To determine this point with more certainty however, and to ascertain whether or no the appearance of the hydrogen was wholly unconnected with the decomposition of the acid, I made two comparative experiments on the quantity of muriate of silver furnished by two equal quantities of muriatic acid, one of which had been converted into muriate of potash by the action of potassium, and the other of which had been absorbed by water; every care was taken to avoid

Spark taken in muriatic gas over mercury.

* When the Voltaic spark is taken continuously, by means of points of charcoal in muriatic acid gas over mercury, muriate of mercury is rapidly formed, a volume of inflammable gas, equal to one third of the original volume of the muriatic acid gas appears, and the acid gas enters into combination with the oxide of mercury, so that water enough is present in the experiment to form oxide sufficient to absorb the whole of the acid.

sources

sources of error; and it was found, that there was no notable difference in the weight of the results.

There was no proof then, that the muriatic acid had been decomposed in these experiments; and there was every reason to consider it as containing in its common aeriform state at least one third of its weight in water; and this conclusion we shall find warranted by facts, which are immediately to follow.

I now made a number of experiments, with the hopes of obtaining the muriatic acid free from water.

I first heated to whiteness, in a well luted porcelain retort, a mixture of dry sulphate of iron, and muriate of lime which had been previously ignited; but a few cubic inches of gas only were obtained, though the mixture was in the quantity of several ounces; and this gas contained sulphureous acid.

I heated dry muriate of lime, mixed both with phosphoric glass and dry boracic acid, in tubes of porcelain, and of iron, and employed the blast of an excellent forge; but by neither of these methods was any gas obtained, though when a little moisture was added to the mixtures, muriatic acid was developed in such quantities, as almost to produce explosions.

The fuming muriate of tin, *the liquor of Libavius*, is known to contain dry muriatic acid. I attempted to separate the acid from this substance, by distilling it with sulphur and with phosphorus; but without success. I obtained only triple compounds, in physical characters something like the solutions of phosphorus and sulphur in oil, which were nonconductors of electricity, which did not redden dry litmus paper, and which evolved muriatic acid gas with great violence, heat, and ebullition, on the contact of water.

I distilled mixtures of corrosive sublimate and sulphur, and of calomel and sulphur. When these were used in their common states, muriatic acid gas was evolved; but when they were dried by a gentle heat, the quantity was exceedingly diminished, and the little gas that was generated gave hydrogen by the action of potassium. During the distillation of corrosive sublimate and sulphur, a very small quantity of a limpid fluid passed over. When examined by transmitted light, it appeared yellowish green. It emitted fumes of muriatic acid, did not redden dry litmus paper,

Muriatic acid gas contains one third of its weight of water.

Attempts to obtain it free from this.

Muriate of lime distilled with dry sulphate of iron,

phosphoric glass, and dry boracic acid.

Muriate of tin distilled with sulphur and phosphorus.

Muriates of mercury distilled with sulphur,

and deposited sulphur by the action of water. I am inclined to consider it as a modification of the substance discovered by Dr. Thomson, in his experiments on the action of oximuriatic acid on sulphur.

and with phosphorus.

Messrs. Gay-Lussac and Thenard have mentioned*, that they endeavoured to procure dry muriatic acid by distilling a mixture of calomel and phosphorus, and that they obtained a fluid, which they consider as a compound of muriatic acid, phosphorus, and oxygen. In distilling corrosive sublimate with phosphorus, I had a similar result, and I obtained the substance in much larger quantities than by the distillation of phosphorus with calomel.

Phosphorus burned in oximuriatic acid gas.

As oximuriatic acid is slightly soluble in water, there was reason to suppose reciprocally, that water must be slightly soluble in this gas; I endeavoured therefore to procure dry muriatic acid, by absorbing the oxygen from oximuriatic acid gas by substances, which, when oxygenated, produce compounds possessing a strong affinity for water. Phosphorus, it is well known, burns in oximuriatic acid gas: though the results of this combustion, I believe, have never been minutely examined. With the hopes of procuring muriatic acid gas free from moisture, I made the experiment. I introduced phosphorus into a receiver having a stop-cock, which had been exhausted, and admitted oximuriatic acid gas. As soon as the retort was full, the phosphorus entered into combustion, throwing forth pale white flames. A white sublimate collected in the top of the retort, and a fluid as limpid as water trickled down the sides of the neck. The gas seemed to be entirely absorbed, for when the stop-cock was opened, a fresh quantity of oximuriatic acid, nearly as much as would have filled the retort, entered.

A white substance sublimed, and a fluid formed.

The same phenomenon of inflammation again took place, with similar results. Oximuriatic acid gas was admitted till the whole of the phosphorus was consumed.

No muriatic acid gas formed.

Minute experiments proved, that no gaseous muriatic acid had been evolved in this operation, and the muriatic acid was consequently to be looked for either in the white subli-

* The *Moniteur* before quoted.

mate, or in the fluid which had formed in the neck of the retort.

The sublimate was in large portions, the fluid only in the quantity of a few drops. I collected by different processes sufficient of both for examination.

The sublimate emitted fumes of muriatic acid when exposed to air. When brought into contact with water, it evolved muriatic acid gas, and left phosphoric acid, and muriatic acid, dissolved in the water. It was a nonconductor of electricity, and did not burn when heated; but sublimed when its temperature was about that of boiling water, leaving not the slightest residuum. I am inclined to regard it as a combination of phosphoric and muriatic acid in their dry states.

Properties of the sublimate.

A compound of dry phosphoric and muriatic acids.

The fluid was of a pale greenish yellow tint, and very limpid; when exposed to air, it rapidly disappeared, emitting dense white fumes, which had a strong smell differing a little from that of muriatic acid.

Properties of the fluid.

It reddened litmus paper in its common state, but had no effect upon litmus paper which had been well dried, and which was immediately dipped into it. It was a nonconductor of electricity. It heated when mixed with water, and evolved muriatic acid gas. I consider it as a compound of phosphorous acid, and muriatic acid, both free from water*.

A compound of phosphorous and muriatic acids free from water.

Having failed in obtaining uncombined muriatic acid in this way, I performed a similar process with sulphur, but I was unable to cause it to inflame in oximuriatic acid gas. When it was heated in it, it produced an orange coloured liquid, and yellow fumes passed into the neck of the retort, which condensed into a greenish yellow fluid. By repeatedly passing oximuriatic acid through this fluid, and distilling it several times in the gas, I rendered it of a bright olive

Sulphur heated in oximuriatic acid gas.

* I attempted to obtain dry muriatic acid likewise from the phosphuretted muriatic acid of Mess. Gay-Lussac and Thenard, by distilling it in retorts containing oxygen gas, and oximuriatic acid gas. In the first case, the retort was shattered by the combustion of the phosphorus, with a violent explosion. In the second, compounds, similar to those described above, were formed.

Phosphuretted muriatic acid distilled in oxygen and oximuriatic gas.

colour, and in this case it seemed to be a compound of dry sulphuric and muriatic acid, holding in solution a very little sulphur. When it was heated in contact with sulphur, it rapidly dissolved it, and then became of a bright red colour, and when saturated with sulphur, of a pale golden colour*. No permanent aeriform fluid was evolved in any of these operations, and no muriatic gas appeared, unless moisture was introduced.

As there seemed little chance of procuring uncombined muriatic acid, it was desirable to ascertain what would be the effects of potassium upon it in these singular compounds.

Potassium introduced into the fluid from muriate of mercury.

When potassium was introduced into the fluid generated by the action of phosphorus on corrosive sublimate, at first it slightly effervesced, from the action of the liquid on the moist crust of potash surrounding it; but the metal soon appeared perfectly splendid, and swimming on the surface. I attempted to fuse it by heating the fluid, but it entered into ebullition at a temperature below that of the fusion of the potassium; indeed the mere heat of the hand was sufficient for the effect. On examining the potassium, I found that it was combined at the surface with phosphorus, and gave phosphuretted hydrogen by its operation upon water.

The fluid deprived of a considerable quantity of phosphorus,

I endeavoured, by repeatedly distilling the fluid from potassium in a close vessel, to free it from phosphorus, and in this way I succeeded in depriving it of a considerable quantity of this substance.

and heated with potassium.

I introduced ten or twelve drops of the liquid, which had been thus treated, into a small plate glass retort, containing six grains of potassium. The retort was exhausted after having been twice filled with hydrogen, the liquid was made to boil, and the retort kept warm till the whole had disappeared as elastic vapour. The potassium was then heated by the point of a spirit lamp; it had scarcely melted, when it burst into a most brilliant flame, as splendid as that of phosphorus in oxygen gas, and the retort was destroyed by the rapidity of combustion.

* All these substances seem to be of the same nature as the singular compound, the sulphuretted muriatic acid discovered by Dr. Thomson, noticed in page 98.

In other trials made upon smaller quantities after various failures, I was at last able to obtain the results; there was no proof of the evolution of any permanent elastic fluid during the operation. A solid mass remained of a greenish colour at the surface, but dark gray in the interior. It was extremely inflammable, and often burnt spontaneously when exposed to air; when thrown upon water, it produced a violent explosion, with a smell like that of phosphuretted hydrogen. In the residuum of its combustion there was found muriate of potash, and phosphate of potash.

I endeavoured to perform this experiment in an iron tube, hoping, that, if the muriatic acid was decomposed in the process, its inflammable element, potassium, and phosphorus, might be separated from each other by a high degree of heat; but in the first part of the operation the action was so intense, as to produce a destruction of the apparatus, and the stop-cock was separated from the tube with a loud detonation.

I heated potassium in the vapour of the compound of muriatic and phosphoric acid; but in this case the inflammation was still more intense, and in all the experiments, that I have hitherto tried, the glass vessels have been either fused or broken; the solid residuum has however appeared to be of the same kind as that I have just described.

The results of the operation of the sulphuretted compounds, containing muriatic acid free from water, upon potassium are still more extraordinary than those of the phosphuretted compounds.

When a piece of potassium is introduced into the substance that distils over during the action of heated sulphur upon oximuriatic acid, it at first produces a slight effervescence, and if the volume of the potassium considerably exceeds that of the liquid, it soon explodes with a violent report, and a most intense light.

I have endeavoured to collect the results of this operation, by causing the explosion to take place in large exhausted plate glass retorts; but, except in a case in which I used only about a quarter of a grain, I never succeeded. Generally the retort, though connected with the air pump at the time, was broken into atoms; and the explosion produced

by

by a grain of potassium, and an equal quantity of the fluid, has appeared to me considerably louder than that of a musket.

Solid compound formed.

In the case in which I succeeded in exploding a quarter of a grain, it was not possible for me to ascertain if any gaseous matter was evolved; but a solid compound was formed of a very deep gray tint, which burnt, throwing off bright scintillations, when gently heated, which inflamed when touched with water, and gave the most brilliant sparks, like those thrown off by iron in oxygen gas.

Its properties certainly differed from those of any compound of sulphur and potassium that I have seen: whether it contains the muriatic basis must however be still a matter of inquiry.

The highly inflammable nature of the compounds probably depends on the muriatic acid.

There is, however, much reason for supposing, that, in the singular phenomena of inflammation and detonation that have been described, the muriatic acid cannot be entirely passive: and it does not seem unfair to infer, that the transfer of its oxygen, and the production of a novel substance, are connected with such effects; and that the highly inflammable nature of the new compounds partly depends upon this circumstance. I am still pursuing the inquiry, and I shall not fail immediately to communicate to the Society such results as may appear to me worthy of their attention.

9. *Some general Observations, with Experiments.*

An experiment has been lately published, which appeared so immediately connected with the discussion entered into in the second section of this paper, that I repeated it with much earnestness.

Experiment of Dr. Woodhouse.

In Mr. Nicholson's Journal for December, Dr. Woodhouse has given an account of a process, in which the action of water caused the inflammation of a mixture of four parts of charcoal and one of pearlsh, that had been strongly ignited together, and the emission of ammonia from them. I thought it possible, that in this case a substance might be formed similar to the residuum described in page 50*; but

* See Journal, vol. XXIII, p. 250.

by cooling the mixture out of the contact of nitrogen, I found that no ammonia was formed; and this substance evidently owed its existence to the absorption of atmospheric air by the charcoal*.

The experiments that I have detailed on the acids offer some new views with respect to the nature of acidity. That a compound of muriatic acid with oxide of tin or phosphorus should not redden vegetable blues, might be ascribed to a species of neutralization by the oxide or inflammable body; but the same reasoning will not apply to the dry compounds, which contain acid matter only, and which are precisely similar as to this quality. Let a piece of dry and warm litmus paper be moistened with the compound of muriatic and phosphorous acid, it perfectly retains its colour. Let it then be placed upon a piece of moistened litmus paper, it instantly becomes of a bright red, heats, and develops muriatic acid gas.

New views of the nature of acidity.

All the fluid acids that contain water are excellent conductors of electricity, in the class called that of imperfect conductors; but the compounds to which I have just alluded are nonconductors in the same degree as oils, with which they are perfectly miscible. When I first examined muriatic acid, in its combinations free from moisture, I had great hopes of decomposing them by electricity; but there was no action without contact of the wires, and the spark seemed to separate no one of their constituents, but only to render them gaseous. The circumstance likewise applies

Fluid acids containing water are conductors of electricity.

* Potash or pearlsh is easily decomposed by the combined attractions of charcoal and iron; but it is not decomposable by charcoal, or, when perfectly dry, by iron alone. Two combustible bodies seem to be required by their combined affinities for the effect; thus in the experiment with the gun barrel, iron and hydrogen are concerned. I consider Homberg's pyrophorus as a triple compound of potassium, sulphur, and charcoal; and in this ancient process, the potash is probably decomposed by two affinities. The substance is perfectly imitated by heating together ten parts of charcoal, two of potassium, and one of sulphur.

Potash decomposed by the combined affinities of two combustibles.

When I first showed the production of potassium to Dr. Wollaston in October 1807, he stated, that this new fact induced him to conceive, that the action of potash upon platina was owing to the formation of potassium, and proposed it as a matter of research, whether the alkali might not be decomposed by the joint action of platina and charcoal.

to the boracic acid, which is a good conductor as long as it contains water; but which, when freed from water and made fluid by heat, is then a nonconductor.

Alkalis &c.
nonconductors
when solid, but
conductors
when fused.

The alkalis, and the earthy compounds, and the oxides, as dry as we can obtain them, though nonconductors when solid, are on the contrary, all conductors when rendered fluid by heat.

Water in mu-
riatic acid gas.

When muriatic acid, existing in combination with phosphorous or phosphoric acid, is rendered gaseous by the action of water, the quantity of this fluid that disappears at least equals from one third to two fifths of the weight of the acid gas produced; a circumstance that agrees with the indications given by the action of potassium*.

Muriate of
mercury in va-
pours passed
through ig-
nited charcoal.

I attempted to procure a compound of dry muriatic and carbonic acids, hoping that it might be gaseous, and that the two acids might be decomposable at the same time by potassium. The process that I employed was by passing corrosive sublimate in vapour through charcoal ignited to whiteness; but I obtained a very small quantity of gas, which seemed to be a mixture of common muriatic acid gas and carbonic acid gas; a very minute portion of running mercury only was obtained, by a long continuation of the process; and the slight decomposition, that did take place, I am inclined to attribute to the production of water by the action of the hydrogen of the charcoal upon the oxygen of the oxide of mercury†.

Muriatic acid
gas attracts wa-
ter from some
other gasses,

In mixing muriatic acid gas with carbonic acid, or oxygen, or hydrogen, the gasses being in their common states as to moisture, there was always a cloudiness produced; doubtless owing to the attraction of their water to form liquid muriatic acid.

* Page 101.

† These facts, and the other facts of the same kind, explain the difficulty of the decomposition of the metallic muriates in common processes of metallurgy. They likewise explain other phenomena in the agencies of muriatic salts. In all cases when a muriatic salt is decomposed by an acid, and muriatic acid gas set free, there appears to be a double affinity, that of the acid for the basis, and of the muriatic acid for water; pure muriatic acid does not seem capable of being displaced by any other acid.

On fluoric acid gas no such effect was occasioned. This fact, at first view, might be supposed to show that the hydrogen evolved by the action of potassium upon fluoric acid gas is owing to water in actual combination with it, like that in muriatic acid gas, and which may be essential to its elastic state; but it is more probable, from the smallness of the quantity, and from the difference of the quantity in different cases, that the moisture is merely in that state of diffusion or solution in which it exists in gasses in general; though from the disposition of water to be deposited in this acid gas in the form of an acid solution, it must be either less in quantity, or in a less free state, so as to require for its exhibition much more delicate hygrometrical tests.

The facts advanced in this Lecture afford no new arguments in favour of an idea, to which I referred in my last communication to the Society, that of hydrogen being a common principle in all inflammable bodies; and except in instances which are still under investigation, and concerning which no precise conclusions can as yet be drawn, the generalization of Lavoisier happily applies to the explanation of all the new phenomena.

In proportion as progress is made towards the knowledge of pure combustible bases, so in proportion is the number of metallic substances increased; and it is probable, that sulphur and phosphorus, could they be perfectly deprived of oxygen, would belong to this class of bodies. Possibly their pure elementary matter may be procured by distillation, at a high heat, from metallic alloys, in which they have been acted upon by sodium or potassium. I hope soon to be able to try this experiment.

As our inquiries at present stand, the great general division of natural bodies is into matter which is, or may be supposed to be, metallic, and oxygen; but till the problem concerning the nature of nitrogen is fully solved, all systematic arrangements made upon this idea must be regarded as premature.

IV.

Extract of a Letter from Mr. J. B. VAN MONS, Member of the Institutes of France and Holland, to the Editor, on Atmospheric Phenomena.

SIR,

Formation of
thunderstorms.

IN a paper which I laid before the Batavian Society of Experimental Philosophy at Rotterdam, I showed, that thunder storms form in the atmosphere spontaneously, and wholly. The diminution of sidereal, and particularly of lunar attraction, suffers the air to sink down, by depriving it of the additional elasticity this attraction imparted to it; this sinking loosens the union between the air and water; the temperature is raised by the separation of the caloric, that served as the medium of this union; and the water separates in some part or other of the atmosphere, forming a cloud. This cloud soon enlarges by the continuation of the same cause, the caloric separates from it in great abundance, and, as the air is a very bad conductor of heat, this can neither diffuse itself, nor be dissipated in the form of light, a modification of caloric into which it is not sufficiently concentrated to transform itself, adopts the state of electric fluid, and decomposes the water of the cloud.

Clouds.

Water convert-
ed into a per-
manent gas by
caloric in a
state approach-
ing to that of
electric fluid,

which is the
cause of per-
manent gasses.

Different states
of caloric.

It is probable, that this effect happens only to a very slight quantity of caloric; and that the portion of this principle, which in combination with air serves to convert water into a permanent gas, is contained in this union in the state of electric fluid, or at least in a state intermediate either to that of heat and electricity, or of electricity and light; which fourth state being incapable of subsisting except in combination, will never be known to us separately, or otherwise than by its effects. This state is the agent, by means of which permanent gasses retain their state. With the bases of these gasses it enters into a chemical union, which can be broken only by an affinity of the same nature.

Caloric alone cannot convert these bases into gas, before
it

it is sufficiently concentrated to assume the requisite elasticity, and then it is in the state of light. Light, though little concentrated, produces this effect, because it has only to lose a little of its elasticity to become electricity, or subelectricity, the fourth modification of caloric; which excess of elasticity it transmits however to the caloric, with which the bases abovementioned are fixed, and which has lost much of its natural elasticity in that fixation. Thus more or less elasticity constitutes all the difference between light, the electric fluid, sublight, and subelectricity, if indeed this exist, and heat. We cannot take a single step in natural philosophy or chemistry, without perceiving the facility with which these agents are metamorphosed one into another; a metamorphosis on which depends a very great number of phenomena.

Their ready conversion into each other the cause of many phenomena.

It is the heat alone that separates in great abundance, and in a distinct part of the atmosphere, which can thus transform itself into electric fluid. That which is produced by the general loosening [*re achement*] of the air, or a certain decomposition of this fluid in its aqueous combination, and which heats the atmosphere, has no occasion to diffuse itself, being generally separated, and it remains heat. Every increase or diminution of the temperature of the air is spontaneous, and not communicated, or conducted by the winds, which are themselves the effects, and not the causes, of changes of temperature, and other alterations that take place in the atmosphere.

Heat partially and constantly separated only becomes electricity.

At every increase of the temperature of the air, the barometer sinks, because the precipitation of water diminishes the elasticity of this fluid: as every diminution of temperature, which always results from the combination of water with air, with fixation of caloric, and the transformation of heat into electricity, causes the barometer to rise by the increase of elasticity which the air acquires. This last effect frequently takes place during rain, when this rain is the excess of water which the air deposits, to be enabled to resume, constantly under the influence of some sidereal cause, that state of serenity, which constitutes fair weather. This rain, or that which falls with a rising of the barometer and a falling of the thermometer, is a rain of recombination

Heat of the air not communicated but spontaneous.

With the rise of the thermometer the barometer falls, and vice versa.

of

of the air in its aqueous combination; and that which falls with a sinking of the barometer and a rise of the thermometer is a rain of decomposition of the air with respect to that combination.

The barometer rises from the increased elasticity of the air, not from its weight.

I cannot easily conceive, how people continue to ascribe to the weight of the air that pressure, which this fluid exerts on the mercury in the barometer; while we see it almost always increases this pressure, when it loses part of its matter, or gravitating power; and diminish it when the air is at a *maximum* of aqueous saturation, or just before rain; and when the bulb manometer, or true aerostatic balance, indicates the ultimate degree of density in the air; and that all other phenomena, both those that occur in nature, and those that present themselves in experiments with the mercurial pump, prove to a demonstration, that the air presses chiefly by virtue of its elastic power, which is increased by condensation, and by the addition of caloric, the matter remaining the same in a closed space; and diminished by rarefaction, and the subtraction of caloric, the matter remaining equally the same, and in the same space; but which is neither increased nor diminished in the open air, but by the association, more or less elastic, more or less solid, of water with air.

Experiments in confined air not applicable to meteorology.

This proves how little applicable to atmospheric phenomena are the results we obtain under our glasses, in which the air is deprived of its free motion, and where this fluid is withdrawn from the effects of rarefaction and condensation produced by sidereal influences; which effects, added to the more or less permanent or solid gassification of water, and the transformation of light and of heat into electric fluid, give rise to all meteoric phenomena, and occasion by their frequent variations the great variableness of the state of the atmosphere.

Formation of clouds.

The first portion of water decomposed into gas, while it changes the composition of the air, and increases the density of this fluid at the point where this decomposition takes place, determines the formation of other clouds, which deposit also electric fluid, and are in part decomposed, and so

Charged with electricity.

on. The electric fluid, that does not combine to gassify the principles of water, charges these clouds by strata of opposite

opposite zones, in the same manner as it charges semiconductors, and as their natural fluid is distributed in the state of charge in nonconductors; and the gasses of water, notwithstanding the lightness of one of them, dissolve in the air as spirit of wine dissolves in water, and remain diffused in the matter of the cloud. Soon, by the condensation of the fluid, or the intensity of the charge, this state destroys itself; the fluid bursts from stratum to stratum, and the water is recomposed by the inflammation of its gasses. The fulguration or flashes of lightning without or almost without noise, and the light of which perfectly resembles that of the electric spark, are the effects of the explosion* of the fluid of that spark; and the flashes accompanied with thunder, or true lightning, which diffuse the same light as the combustion of hidrogen and oxigen gas, are those of the combustion of the gasses of water. These two sorts of lightning alternate with each other, because the decomposition and recomposition of water take place alternately. The rolling of thunder arises from a succession of partial inflammations, in proportion as the strata oppositely electrified confound their two states. The sounds too are different; that of the fulgurations being acute, quick, snapping; and that of the fulminations heavy, dull, rolling; and, from their analogy to the sounds produced by electric sparks and the combustion of hidrogen and oxigen gas in our experiments, may easily be referred to the phenomena, to which they belong.

The principles of water diffused in them.

Discharge of the clouds.

Lightning of two kinds.

Thunder of two kinds.

The sound of the combustion is more intense, because a vacuum is formed, which is instantly filled, and more than filled, by the vapour of water, that acquires a state of considerable expansion. When once the rain has begun to fall, and the work of the storm is set a going, it proceeds of itself, or has no longer occasion for the formation of fresh clouds to keep it up; the caloric that separates from the combined gasses transforming itself into electricity, which in its turn decomposes a portion of water; so that the work of the successive decompositions and compositions continues by the effect of its alterations, and is kept up of itself, till all the water diffused through the surrounding air by vaporization is condensed there, and resolved into rain; or till,

Cause of the intensity of the sound.

Progress of the storm.

by

The air growing cool indicates its cessation.

by the separation of the fluid, and its conveyance to the Earth, in consequence of its great condensation, as well as of the water of the cloud being again taken up in solution by the air, the storm ceases before this has happened. The water of clouds being again taken into solution by the air occasions a cooling of the air, and presages a definitive cessation of its stormy state; while the heating of the air, or continuation of its high temperature, denotes the continuation of the decomposition, and is always followed by a recommencement of the storm.

Hail.

Hail arises from a strong fixation of caloric, which transforms itself into electricity, to gassify the principles of water; and sometimes from a too copious combination of the same caloric converted into electric fluid to reunite the water with the air; or from the same conversion of caloric to reinforce the thunder, which endeavours to explode toward the Earth. This explosion of the thunder takes place either after a considerable recombination of water, or when, the greater part of the water of the storm being dispersed, the electric fluid remaining no longer finds any thing to which it can adhere, concentrates itself in a point, and acquires elasticity enough to overcome the opposition of the air, and rush toward the Earth, or some prominent points on the globe. As this passage of the thunder toward the Earth is not solicited by a state of subtraction, opposite, or negative charge, the course it follows is neither direct, or the shortest possible, nor determined to a given point; but its course is uncertain, irregular, and in some measure vague, bursting from one substance to another, even striking the ground and separating from it anew, without any other cause than the difficulty of diffusing or decomposing itself.

Cause of the great mischief done by lightning.

To this difficulty of resuming an equilibrium, which it finds no where broken, or of diffusing itself in a point of subtraction which no where exists for it, are owing the extraordinary effects of the explosion of thunder, and the incalculable means of destruction, with which we see it act; and that even when it has already arrived at the ground, where it ought to be able to diffuse itself, it still vaporizes water with great force, splits stones, &c. To the same cause is owing, that it proceeds so slowly, that it so long retains

retains its state of sparkforming concentration, and that it so easily fuses and inflames substances, staying long at each point of its course, and transforming itself easily into light and heat. One portion of the electric fluid separated during a thunderstorm transforms itself into light, and is dissipated in space, at every explosion of a spark, or of a fulmination of combustion. The sound of the thunder that bursts toward the Earth is very different too from that of rolling thunder, and perfectly resembles that of the discharge of our electrical batteries. The common people readily distinguish it, and denote it by the name of falling thunder. The opposite winds that blow during a thunderstorm, and are even contrary to its direction, are the natural effect of a strong condensation of the aqueous part of the atmosphere.

Two sounds of thunder.

Opposite winds during a thunder storm.

A thunderstorm then does not arise from an accumulation of hydrogen gas extricated from the Earth, from which none is extricated, and rising to the superior regions of the atmosphere, whither it does not ascend; this gas never being extricated in its pure state; and that which is extricated in combination with a combustible substance, whether phosphorus, sulphur, or carbon, being burned by a concurrence of action on the part of these combustibles as soon as it comes into contact with the air, and no experiment having ever demonstrated the existence of the least bubble of hydrogen gas in the air at any elevation whatever. Besides, the hydrogen gas we set free in the air does not ascend in it in consequence of its greater lightness, or less specific gravity, but becomes incorporated with the air with which it is in contact, remains adherent to it by an affinity of penetration; and even does not diffuse itself in it without difficulty, and in some time, when the air is perfectly at rest. Nay more, I have strong reasons for believing, that, at the time of great assimilations of water, the affinity of the air for this fluid determines the direct combustion of hydrogen gas by the air, without the intervention of any other inflammable substance.

Hydrogen gas does not ascend from the earth into the upper regions of the air.

Hydrogen gas alone burned in the air.

The rain too is not the consequence of the condensation of aqueous vapour by cold, since the fall of rain always precedes the cooling of the air, while an increase of the temperature

Rain not aqueous vapour condensed by cold.

temperature of the air always precedes rain: water then is dissolved by the air, or rather associated with the composition of the air by the intervention of caloric in the state of

A fourth of the weight of the atmosphere owing to water.

electricity, and this in so large a quantity, that it forms almost a fourth of the weight of the atmosphere. I give in the paper, to which I have alluded above, the facts and experiments on which this calculation is founded; but these facts are not very numerous, and almost all synthetical, that is of addition, or composition, and but few analytical, or of subtraction or decomposition; the air being of all known bodies that which has the greatest affinity for water to a certain point of saturation, of which there are very many degrees, and very distinct, from the nature of the affinity that limits them; so that, without decomposing it, we can scarcely separate the water from it, partly, no doubt, on account of the form of the air, which it faithfully preserves, and which prevents us from retaining it to separate it. And

Difficulty of the synthetical experiments.

the difficulty of synthetical experiments depends on this, that, in removing the water by decomposing it, we cannot prevent the air itself from being decomposed with respect to its oxygen, all the processes we must employ for this purpose being of the disoxygenizing kind, not excepting the electric fluid, which determines the condensation of oxygen by azote. Nothing then is more difficult, than to obtain, for the purpose of synthetical experiments, air deprived of

Best method of depriving air of water.

its water to a certain point; and the method, that has succeeded best for this purpose, is the disengagement of muriatic gas from a very dry muriate, by means of highly concentrated sulphuric acid, in confined air.

Causes of mistakes in determining the proportion of oxygen in substances.

I need not observe to you, how many mistakes in determining the proportions of oxygen in burned substances must have arisen from the great quantity of water, that makes part of the air, which becomes solidly fixed in these substances, and serves as an indispensable medium of the combination of oxygen with the bodies it burns. To this large quantity of water in the air are owing those spontaneous and heavy rains, which frequently fall in an atmosphere, that was perfectly serene and tranquil a moment before.

Caloric proper to the solar

The caloric, that under its different forms is incessantly ascending in the air, without ever returning to the Earth, being

being a substance that belongs to the atmosphere of the Sun, and is foreign to ours and those of other planets; at which it arrives only by virtue of the great elasticity it possesses when in the state of light, and where it is retained only by its adhesion to substances that belong to these planets; must resume the state of light, the moment when, having arrived at the utmost limits of these foreign atmospheres, and being disengaged from the substances that can no longer follow it, it returns to that which is proper to it, and there takes a centripetal motion, or movement of approximation to the Sun; which, being a perfectly transparent and elastic substance, occasions it to take an opposite course with the same velocity, with which it rushed upon it, which must occasion a perpetual circulation of light between the Sun and those globes, that make part of its system.

atmosphere
alone.

Perpetual cir-
culation of
light.

If this were not the true state of things, there would be an incessant accumulation of caloric, that would soon change the face of these globes; while in this hypothesis the equilibrium is scarcely ever interrupted. These globes then would not be visible but from the extreme limits of their atmospheres, and where the caloric, separated from its combinations, is transformed into light: and the opacity of a globe would not at all prevent this effect, in which the globe itself would not interfere; which would make a wonderful difference in the calculations, from which we have determined the apparent magnitudes of the celestial bodies; as in this case their magnitudes would have been calculated from the extent of their atmospheres, and by no means from that of the globes or celestial bodies themselves; and the light, which renders these bodies visible to us, would not be reflected light, but light extricated from them, or returning toward the Sun. It is to be understood, that this extrication cannot take place, except as far as the atmosphere faces the Sun, and is under the direct influence of its attractive power; otherwise the light extricated would diffuse itself through space; take a course different from that to the Sun, and not reach the atmosphere of that celestial body, where alone it can resume its character of light. Nothing prevents the light in this return from traversing

Otherwise an
accumulation
of caloric would
change the face
of things;

and our astro-
nomical calcu-
lations would
be erroneous.

The presence
of the Sun ne-
cessary to at-
tract this light.

other atmospheres. It is by the light refracted in this passage, that we see the globes from which it emanates.

I am, Sir, with great esteem,

Yours, &c.

J. B. VAN MONS.

V.

Remaining Proof of the Cause of Motion in Plants explained; and what is called the Sleep of Plants shown to be Relaxation only. By Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Cause of motion in plant.

No feeling or volition in plants.

Spiral wire.

ANXIOUS to complete the proofs of that idea suggested in my last paper, concerning the motion of plants; and to show, that I should not have endeavoured to call the attention of the public to this subject, had I not possessed what appeared to me to be the most incontrovertible arguments in its favour, with the most solid reasons for believing, not only "that this leatherlike substance and the spiral wire are the cause of motion in plants," and of every degree of irritability (which I was at first fearful of advancing), but that "they are also the cause of what has been mistaken for the sleep of plants." In short, this appears to me perfectly to explain all that has hitherto been considered as *feeling* or *volition* in plants, and to resolve it into mechanical power; and the complete management of the spiral wire. The interior formation of plants, when duly magnified by the solar microscope, proves the vegetable world to be composed of machines governed wholly by light and moisture; and dependant on these causes for motion.

The spiral wire may be considered as a *secondary cause*, acted upon by the *two first*; and by its means all the movements of the plant are made, the flower opens and shuts in the morning and evening, the leaves turn, or the creeping plants

plants wind in their regular order. Nor can the flower opening at a *different time of the day*, or *turning in a different manner*, militate against the argument; as the constant effect of strong light and dry weather is to *contract* the wire; that of darkness and moisture, to *dilute* it; and it depends wholly on *which way* the spiral wire is placed, whether its dilating shall *open* or *shut* the flowers: as in mechanics, the same spring may be made to turn to the right or to the left, to open or shut a box. Most of the flowers I have observed, that close at noon, are extremely limber in the corolla, which is formed only of a double cuticle, *without pabulum*, and soon overcome by heat; and when this is the case, relaxation directly takes place. Such is the convolvulus *nil*, the hesperantha cinamomea, the tiger plant, the evening primrose, &c. The great Author of nature shows in all his plans a simplicity, that must hourly strike the dissector of plants, and prove how much more ability is necessary to produce such *simple mechanism*, than to invent our more *cumbrous machines*.

Spiral wire regulates the opening of the flower.

Mirbel, one of the latest and best of the French botanical writers, though his work is one of the first compendiums of the science, has greatly mistaken the subject, merely from not having *sufficiently magnified* his specimens. He says, "if the spiral wires were common to most plants (which he does not believe) they could not in any way promote the motion of plants, because they are confined in a case which cannot stretch." I shall give an exact drawing of the *case*, which will I think plainly prove it was made for no *other purpose*: but to see this it is necessary to place it in the solar microscope, and Mirbel did not use one. I contrived to measure its increase by taking it out of a leaf stalk, and placing it between a double pair of pincers, which were laid in a groove, moving them by means of a thread over a very little wheel. They were drawn with a delicacy no hand could imitate, and it stretched without breaking (by moistening it) from 1 inch and a quarter, to 2 inches wanting 2 tenths, but it was after being apparently much contracted by light in the microscope. As to the spiral wire it is apparent, that it may be drawn to any length. A larger magnifier would also have convinced Mirbel, that the case

Case of the spiral wire

made to stretch.

is of so thin a substance, or rather I believe I should say of so loose a one, as plainly to be intended to dilate and contract; a few very thin vessels, interlaced with an extremely fine spiral wire, composes it, as will be seen in the plate: while the larger spiral vessels fill up the case in an irregular manner, the nourishing vessels forming a regular circle of tubes round it, and, to complete the contrivance, the midrib of the leaf is formed also to contract and dilate a little with perfect ease, even in the hardest leaves, as the *laurel*, &c. But in some leaves there is a curious contrivance to lengthen it in the bosom of the leaves just where the bud is concealed in its *first birth*, as in the *ash*, *plane*, &c. The peach, and most of that order, will stretch the midrib of the leaf far more than is necessary for any succeeding motion: and if any person could doubt the power of the spiral wire to draw up, and of course turn the leaf, he has but to take a leaf so drawn from a nectarine, or peach; for it seems impossible that it should not be seen, that it is drawn from the inside of the midrib, and not from the gathering up of the cuticle of the leaf, which has been suggested. I have seen in the geranium the spiral wire to have stretched the case with such violence, it could not return to its usual size, but has remained in a spiral form: which shows however how easily the spiral wire acts on the case.

Spiral wire contracting or dilating.

Mid rib of the peach leaf stretches.

Different parts stretch in different degrees.

It may be thought, that it was not necessary the stalk should dilate itself, provided the part within did; but nature finds its account in this arrangement, the leaf stalk could not turn with ease, did not one side of it contract, to wind it round. They have all in this respect their *appropriate proportions*, the spiral wire stretches to any degree wanted, the case a great deal less than the former, and the outward cuticle has only flexibility enough not to impede the continual irritability of its inward spiral wire.

Extreme effect of the spiral wire.

In my first letter on this subject, I showed the effect of the spiral wire on plants in general, selecting those that were only *commonly affected* by it, that I might not be accused of favouring my subject, in order to conceal any weakness the argument might possess. It is truth alone I seek, and I can have no other attachment to it but supposing it so; my eyes and my microscope must grossly deceive

ceive me, before I deceive others. I have now to present three of the most sensitive plants I am acquainted with, in order to show the *full strength* of the spiral wire, and of that sort of leathery substance of which it is composed. The first is the Indian grass that conducts the hygrometer made by Captain Kater; the second is the thorn of the nettle, which is certainly made of the same leatherlike substance; and the third is the mimosa sensitiva, the bag of which plant is also of the same nature, though infinitely thinner. They are all governed by this substance turned into a spiral wire, and the same substance in another form. When thicker it is certainly infinitely stronger, as it proves by the awn of the grass, which is so powerful, though much of it has lost its spiral wire. There is a water plant that sends up its flower by the same substance, and which I have not been able to procure; but I shall be satisfied with showing the dissection of these plants, perfectly convinced they will be thought sufficient to prove, that this *substance* is the *cause of motion in plants*.

The first is an Indian grass, but the only part sensitive is the awn, which is formed of this leatherlike substance, infinitely thicker and stronger than the usual spiral wire, and its untwisting would be certainly capable of regulating a much more powerful instrument. The awn is formed of two apparently flat pieces, with a cylindric hollow running through the middle, which is filled with a thick spiral wire, but which I have found in only two pieces: the rest (I suppose from long keeping) must have decayed. Each side is bristled as the awns of grass generally are, but I never could perceive, that these bristles added any sensitive power to the awn, though from their resembling those in the sensitive plant I expected to find that they did. I therefore deprived both plants of this ornament; but I could not perceive any difference in their sensibility, and in neither specimen are they twisted. It is quite wonderful to see the strength with which this wire twists and untwists. It is only the very finest part, that can be placed in the solar microscope, without breaking the glasses between which it is laid, though not three tenths of an inch in length. What is extraordinary is, *that so made*, it will continue to *untwist* into two different

Formation of the awn of the Indian grass.

The untwist

ing the awn of the grass. different threads, and I doubt not at last separate into as fine a wire as runs in the mimosa. The hygrometer made of it is the best that was ever invented, and indeed it is difficult to conceive one more sensible, since at nearly an inch distance the moisture of a finger will cause it to make one or two revolutions of 100 parts each. The figure of the instrument was given in your Journal for last July.

Formation of the sting of the nettle.

The next plant is the nettle, irritable only at the *awn* or *sting of the plant*. It is a long pipe with a bag at the end, divided into two parts; the smaller, in which is enclosed the bag of the poison; and the larger, which is below it. The whole bag appears to be formed of the same leather-like substance as the awn of the grass, and to be, in proportion to its size, equally affected by light and moisture. The moment the upper part of the pipe is touched, the under part of the bag *whirls up*, breaks the poison bladder, and throws its contents violently up the pipe, burning the person who touches it. This is instantaneous; and so susceptible is it, that the light thrown on it in the solar micro-

Effect of light on the sting of the nettle.

scope has exactly the same effect as the touch. The liquor is protruded with a force quite wonderful up the pipe, till it issues at the minute aperture of the point; but before it does so, the pipe is bent down with a jerk by the spiral wire, exactly as the leaf stalk of the mimosa sensitiva bends to the touch. They are managed exactly by the same force, and governed by the same powers, *light* and *moisture*. This description of the nettle will account for its not stinging when pressed hard; the pipes being then broke, and the liquor of the nettle, mixing with the poison, dilutes it so completely, that it has no longer any effect. It will be seen, that the spiral wire is carried round the bag, and that it is drawn together by the contraction of the wire: and to complete the likeness of the three plants I must mention, that the nettle lays down its large stings every evening just as the sensitive plant does its branches, and that the awn of the grass also untwists towards evening.

Lays down its stings at night.

Sensitive plant.

I shall now turn to the mimosa sensitiva, which has less of real strength, but more mechanism than the other two. Its motions proceed from the same cause, which is not only the spiral wire, but a bag of the same leathery kind, that contracts

contracts and dilates: but the plant seems much more to depend on the spiral wire. It is impossible not to be struck with astonishment and admiration at the beauty and delicacy of the contrivance, which is far more artificial than the works of nature generally are; and I know not a plant, that has taken me so much time, and given me so much trouble, to develope the different joints, pulleys, knots, and bolts. I have long perceived, that a plant is sensitive in proportion to the *tight manner* in which it is twisted, and the short distance between the *knots*: now there is scarcely in this plant $\frac{1}{16}$ of an inch between the knots; and the spiral is twisted as tight as *possible*. I could not persuade myself the sketch was exact, till I had from different specimens drawn it twelve or fourteen times; but I have exposed the greatest part to the view of so many friends, that I think I may truly answer for its being exactly sketched after nature.

At D. fig. 1, Pl. IV, are the springs that govern each leaf, Mechanism of
the leaf. *dd* is the stalk. Each leaf has a base *cc*, which serves to concentrate the spiral wires. These passing over in every direction, being drawn *through the narrow part of the stem* by the strings *bb bb*, press the stem together; and, when touched, lay the leaves one on the other the whole way down the leaf stalk. But before the stimulus is applied; the stem is flattened in a contrary direction. The ball of the leaf is hollow, and filled with oil. The parts *ee* and *pp*, Pl. III, fig. 8, are made of that leathery substance, which forms the cuticle, and is contracted by the light in the solar microscope, just as the bag of the nettle is acted upon, or the twisting part of the grass. The parts *ee* contain the oil, which serves to lubricate the knots (I suppose) and enable them to slip over each other; beside probably acting some important part in the formation of the various gasses and juices in the composition of the plant. When touched the whole string relaxes at *oo*, and lets the branch fall. This it would also do at *m*, if it was not supported by the wood vessels turning into the leaf. Fig. 2, Pl. IV, is the part *ee pp*, uncut, and in its natural state. The sort of bolts are retained in their places by the wood vessels which cross them in every direction; as in trailling plants they do, to defend the *bud from accident*. I should have placed them

them in the sketch, had I not been fearful, by mixing them with the spiral wire, to make such confusion, they would not be known one from the other. But it is easy to understand, that by crossing vessels they are retained in their present situation.

Seminal leaves have no spiral wire.

I must now mention a circumstance, which helps greatly in my mind to prove, that the spiral is the cause of motion. It is that in the *seminal leaves* there is no spiral wire; and in the seminal leaves there is no *motion* whatever. In describing the spiral wire I did not mention the case, in which it is confined, because I wished at the same time to give a sketch of it to avoid confusion. It will now be found in Plate IV, fig. 4.

Drosera acaulis very curious.

I might add to the three plants I have given many others, in which the spiral wire distinguishes itself. The *drosera acaulis* is entirely governed by the spiral wire, which enters the hairs, or rather *arms* of the leaf, and the moment a fly touches it, it collapses, confining it within its circle; or should the fly escape the first arm, the point leaves so viscid a humour, that the next is sure to be caught. This is infinitely more curious in its formation than the *dionea muscipula*; which is also governed by the spiral wire turning over a ball like the *mimosa*, and drawing the leaves together in the same manner. But *drosera* is managed in a more peculiar way, and well worthy a drawing, which I will give in my next.

Leatherlike substance the same matter with the spiral wire.

I hope to be perfectly understood, in giving an account of that which regulates the *motion of plants*, that the *leatherlike substance*, and the *spiral wire*, are the *same matter*. The thickness of the first balancing the force the second gains by its spiral form; and the latter gains much strength also from the case that encloses it.

Recapitulation of the proofs.

Before I close this letter you will excuse my recapitulating the proofs brought forward in it and the former. The spiral wire is found in every leaf that has *motion*; in no leaf that does not move; in no *firs*, *grasses*, *sea-weed*, except *confervas*, and the *confervas* alone of all the tribe have *motion*. It is found in no *chenopodiums*, *salsolas*, or ice plants. In leaves that have no other motion than toward the *stem* and back again, the spiral wire occurs only in the midrib of the leaf;

leaf; in all others, it is found even to the smallest spire, The spiral wire is made to stretch, and so does its case. They both contract and dilate in the solar microscope. The spiral wire is found in all the sensitive plants in great quantities, and in every part except the *seminal leaves*; and it is the seminal leaves *alone* that have no motion. I may add, that, when the spiral wires were divided, the leaf would not turn. I think it is hardly possible, where positive evidence is not to be had, to prove a fact in a more direct manner; and that I may say, that plants have spiral wires, which, contracted and dilated by light and moisture, are the cause of all motion in plants.

The sleep of plants is nothing more than the dilating and *Sleep of plants.* lengthening of the spiral wire from the evening moisture; and I think the very appearance of it proves it to be so. Does not every violent rain, long continued, produce the same effect? Does not the common acacia, when wetted by continued rain, drop her leaves as at night? So does the *gleditsia triacanthos* also with even less moisture. The *æsculus hippocastanum* droops as soon as the leaves are old, and begin to decay. Every plant drops its leaves before the leaves go off. It appears to me, that there is no expression in the human countenance more easy to be understood, *Strength and debility in plants.* than the expression of strength and debility in the appearance of plants. Nor did I ever see a plant close its leaves without showing even an excess of debility in every other part.

In one of my two former letters I mentioned two in- *Plants have no volition.* stances of apparent volition in plants, to show how many things of that sort happen, to mislead the judgment: but I have now too often pursued them, till undeceived, through such a course of experiments, perpetually renewed to gain nothing but disappointment, that I am now most absolutely convinced that all plants are merely machines, governed by light and moisture; and that every idea of their sensibility, or of their volition, is only a proof, that we too often let imagination run away with our judgment. Mechanical power is sometimes so delicately managed, that it is difficult to trace it even with the solar microscope; especially as that is of no use till the specimen is most delicately dissected, and placed

placed in order for this purpose. Such fine instruments are required, that a surgeon's collection has scarce need of more variety than the botanical dissector.

Sensitive plant
does not give
out much ox-
igen.

On immersing a specimen of the mimosa in a cylinder of water, to see whether it produced much oxygen, I found what I expected, that the oil in the plant would not permit the water to approach or touch it. It lay quite hollow from the plant, which closed the moment I placed it there; but the next morning, when the sun shone full on it, it opened, and remained thus till night, when it again closed. Again the sun opened it, but from that time for near a fortnight it remained open, and when I took it out of the water it had lost all power of motion, and had I suppose dilated the wire till no longer capable of stretching farther. It gave not much oxygen.

I should apologize for the extreme dryness of this letter; but to explain the formation of any sort of a machine can only be done by the most simple and clear method, and nothing is less entertaining than such a discourse. To advance a few steps nearer to truth is however a certain gain, and if I have made out my proposition to the conviction of those who study the subject, I am satisfied. Those who possess a solar microscope I can only advise to follow me in my experiments, for without seeing it, it is impossible to conceive the amazing effect of light on plants, or almost to imagine what are its powers.

No perspira-
tion in plants.

I will not finish this letter without adding a few words on the perspiration of plants; a subject I have so repeatedly brought forward. I mentioned in one of my last papers, that, on placing a plant in a growing state, under a glass, I put under the glass a paper doubled a few times so as to raise the glass $\frac{1}{8}$ of an inch from the stand; to introduce under the glass the *smallest* quantity of air possible; just enough to prevent the air from *stagnating*, and the plant from becoming *sick* or *discomposed*, and the plant gave out no moisture. This gave me the idea, that it really was the sickness of the plant, which caused the degree of moisture *Duhamel* talks of, but which is certainly *excessively exaggerated*. But for a farther proof I enclosed a large plant in a silver paper case, with a hoop very-thin that would preserve it

it from pressing on the plant, and weighing the whole, covering the plant, and tying it close at bottom, I left it six hours. The difference of the weight, when I took it out, was *half a grain*; nor could I feel the least moisture on the paper. On breaking off different branches, and blinding my eyes, I was always able to discover the branch dismembered from the other, provided it had been so for half an hour, from the moist feel of the leaves, that can be compared only to the death-like touch of a dying person. Most plants have this when in sickness, and I am persuaded when confined in stagnant air. I think I may therefore finish *this subject also*, and say, that there is *no sensible perspiration in plants*, and that *I very much doubt* whether there is even *insensible perspiration*: *if there is, it is most trifling*.

Moisture
caused by
sickness.

I am, Sir,

Your obliged servant,

Cowley Cott.

AGNES IBBETSON.

Aug. 20.

Explanation of the Plates.

Plate III, figs. 1, 2, 3, a sting of the nettle in its different states. Fig. 1, its perfect state, when placed in the solar microscope, and before it stings: *z* the bag of poison: *x* the spiral wire.

Explanation of
the plates.

Fig. 2, the sting after the poison has been thrown to the point: *x* the spiral wire contracted. This is not drawn up at night, when the stings bend, but only when it is touched. If however a mirror be held over them, the poison is thrown up directly.

Fig. 3, the sting of the nettle very much broken.

Fig. 4, untwisted Indian grass greatly magnified, showing the manner in which it is formed. This is the first grass foreign or English, in which I ever found the spiral wire; but I doubt whether it runs through the whole of the grass.

Fig. 5, the awn of the grass.

Figs. 6 and 7, the grass twisted.

Fig. 8, a longitudinal section of the leaf stalk of the *mimosa sensitiva*, the middle part containing five cases full of spiral wire, and *e.c.* extrem t containing only three.

Pl. IV, fig. 1, a leaf of the *mimosa*.

Fig.

Fig. 2, the extremity of the leaf stalk, at *pp*, Pl. III, fig. 2, undivided.

Fig. 3, horizontal section of the stem of the sensitive plant.

Fig. 4, part of a case full of the spiral wire much more magnified than in fig. 8 of Pl. III. In many plants it is much thicker, but always loose: that is, it is formed exactly like this, but doubled, or trebled, I imagine to preserve it from the effect the moisture of the nourishing vessels might have on it.

Fig. 5, the spiral wire still more magnified.

VI.

A curious Property of Single Repetends. In a Letter from
W. SAINT, Esq.

Cromer, Norfolk, Aug. 10th. 1809.

To Mr. NICHOLSON.

SIR,

Single repetends divisible by any number, except 5 and its multiples.

A Friend of mine, some time since, in the process of an arithmetical operation, observed, that any repetend digit, as 111111 &c., or 777777 &c., would divide by the odd numbers 3, 7, 9, and 11; and supposing it probable, that such repetends would divide by *any* odd number, 5 and its multiples excepted, he had the patience to try all such divisors from 1 to 151, and found them to succeed, by taking a sufficient number of digits for the dividend, which, he observed, never *exceeded* the number denoted by the divisor. This property of numbers my friend submitted to me for demonstration, and as it is certainly a very curious one, I thought it probable, that it might not be unacceptable to many of your readers. I have accordingly sent it herewith, in the form of a proposition, accompanied with a demonstration.

I am, Sir,

Your obliged and humble servant,

W. SAINT.

Proposition.

PROPOSITION.

Proposition.

EVERY odd number, except 5 and its multiples, is a divisor of a repetend of any of the nine digits; and the number of digits necessary to form the dividend will never exceed the number expressed by the divisor.

Demonstration.

First it is evident, that, if we can prove the truth of this proposition for a repetend of units, it must necessarily be true also for a repetend of any other digit, since such repetend would be a multiple of a repetend of units. Demonstration.

Again if the former part of the proposition be true, the truth of the latter part also easily follows; for if 111111, &c. be divisible by any number D , no remainder can recur, till after the remainder 0 has occurred; since, if any remainder recurred before the division terminated, the operation would proceed with *precisely the same figures* as when that remainder first occurred; and thus this remainder would recur again, and so on ad infinitum; and hence the division would never terminate, or 111111 &c. would not be divisible by D , contrary to hypothesis. Now since all the possible remainders, which can occur in dividing 111111 &c. by D , will be between D and 0 inclusive, therefore there can be but D different remainders; and since, in the operation of division, each figure in the dividend will give one remainder, therefore D figures in the dividend will give D remainders. Hence in dividing 111111 &c. to D places of digits by D , all the different remainders, which can take place, will occur; and therefore, if the dividend were to consist of more digits than are denoted by the *divisor*, some one or more of the remainders would recur; and hence if the division did not terminate *previous to this recurrence*, it would never terminate, but would go on ad infinitum. Consequently, if 111111 &c. be ever divisible by D , it must be so when or *before* the dividend consists of D digits. We say *before*, because, though the remainder 0 might not occur precisely at the *end* of D digits, yet, from what has been shown above, if that remainder

remainder ever occur, it must necessarily do so before the dividend exceeds D digits.

Let therefore 111111 &c. to D digits, when divided by D , give q for a quotient and r for a remainder, now since this remainder will recur again, whether 111111 &c. be or be not divisible by D , if the number of digits D in the dividend be increased, let therefore 111111 &c. to $D + d$ digits, when divided by D , give Q for a quotient and r for a remainder.— In the first case we have 111111 &c. to D digits $= Dq + r$, and in the second case we have 111111 &c. to $D + d$ digits $= DQ + r$, the difference of these equations gives 111111 &c. 000000, &c. $= DQ - Dq$, where it is evident the units will consist of d digits and the ciphers of D places, whence

$$Q - q = \frac{111111 \text{ \&c. } \dots \text{ to } d \text{ digits } 000000 \text{ \&c. } D \text{ places}}{D}$$

$= a$ whole number, where the divisor D is any number even or odd. Now the 111111 &c. to d digits must, without the ciphers, be divisible by every odd number not greater than the divisor D (5 and its multiples excepted) for if it were not, whatever were the remainder, suppose R for instance, we should have R 0000 &c. to D ciphers divisible by an odd number not a multiple of 5, which is impossible; moreover 111111 &c. to d digits, will not divide by 2, or by 5, or by any multiple of these numbers, since no multiple of 2 or 5 can terminate with 1: hence 111111 &c. to d digits is divisible by any odd number, except 5 and its multiples, where d the number of digits in the dividend can never be greater than D the divisor, since the recurrence of any remainder must take place in D digits of the dividend.

Q. E. D.

VII.

On the Use of Iron for Stairs, and instead of the Timbers of Houses, as a Security against Fire. In a Letter from Mr. BENJAMIN COOK.

To Mr. NICHOLSON.

SIR,

IN a former paper I threw out some loose hints on the advantage of employing iron in various articles of furniture,

Iron recommended for stairs.

as a substitute for mahogany and other expensive woods. I will now add to it a mode of substituting it in the place of oak, and other less expensive woods.

The chief use I would recommend it for is in stairs, and stair cases, but especially in the metropolis, where so many fires are constantly happening, and where so many lives are annually lost by them; where so many plans have been devised for fire-escapes, and so few, if any, that have ever answered the end.

I have long wondered some plan has not been thought of, which provided security within doors, instead of waiting for precarious assistance from without. It is not so easy to introduce a remedy, such a remedy I am now proposing, into houses already built; either from a parsimoniousness of the owners, or from a fancied security in the idea, that with them there is no danger, and therefore they will not go to the expense of adding a new flight of stairs; which beside the expense, will be attended with much trouble and confusion. The other class, that are likely to hinder the adoption of the remedy, are those that are not able to go to the expense of the alteration. But those persons that could afford it, and wished to provide for the danger of fire, if a probable remedy was shown them, might certainly do it; and as houses are continually altering, and new ones constantly being erected, certainly it would decrease the evil, and be introducing, if but slowly, a system that in years would increase, and be of essential utility.

The remedy I mean is stairs and stair cases made either of cast and sheet iron combined, or cast iron only. The framing for the stairs, to which the boards are nailed in the present mode, might all be cast, and screwed together. Of course this framing would be considerably lighter in appearance, than if made of wood. The front and top of the step, if made of sheet iron, might be attached with six or eight screws, to the cast iron framing; and in order to give it a neat finish, a light bevelled moulding might run all round the front of every step, and the jointings be neatly screwed to it with small screws, with heads countersunk into the mouldings.

Security against fires should be provided within doors.

This would be had from iron stairs.

Mode of constructing them.

But if the front and top of the steps were cast in plates, which

which I think the cheapest and easiest way, the framing might be cast with sunk edges; so that the front and top of the steps would just fit into the grooved framing, and four or six screws would fasten them in a few moments. All the tops and fronts, when cast in a mould, would fit in the framing; and all the framing being so cast to fit, a flight of stairs would soon be put together; the plates might all be cast light, and, when all screwed together, would appear a handsome mass of iron.

May be made
very hand-
some.

They who are unacquainted with the method of casting may suppose, that the work would leave the sand rough and uneven; but, if it is cast in fine sand, it will be level and uniform, and be ready for screwing together, the surfaces will be as regular as stone, when put together, and not so liable to wear smooth, and endanger a person to slip off, in coming down stairs. Such stairs will certainly be much handsomer than stone, and of half the price, or less: with this advantage, the railing may match, and be made of cast iron also.

They would appear very beautiful, if well painted, to imitate mahogany, as also the railing, which might be cast in very handsome and various fanciful patterns. There would be much scope for genius and fancy in devising and executing the staircases and railings, as almost any device, almost any antique figure, or gothic scroll, might be tastefully introduced, forming an elegant, indeed I might venture to say, if expense was not the object, the most beautiful, and certainly the most durable, staircases, that can possibly be formed.

Common
staircases.

Common staircases of iron would certainly be made as cheap, or cheaper than of oak; and I think, if a manufactory was to be established, and a regular trade made of it, they might afford them as cheap as any kind of wood, and a great deal more work might be put in them, as far as concerns the ornamental part: for the same cast, that formed only straight lines, would, by varying the mould, at the same expense, form the most beautiful specimens of antiquity. Therefore wood cannot be brought into comparison with it on the score of taste, nor can price be admitted as an objection to its introduction. Besides, if painting was looked upon

upon as an expense, they would always look well if brushed with black lead; and, as all houses, except the houses of the lower orders, have carpets up the stairs, the tread would be quite as pleasant as on stairs made of mahogany; and in case of fire, a safe escape would always be ready. Dreadful must be the situation of those persons, who, waked by the cry of fire, rush to the landings, find the lower rooms are burning; the staircase blazing and falling; and no escape left but the dreadful one of precipitating themselves from a window, running the risk of being dashed to pieces, or of remaining in the house, to perish in the flames; when, if the stair case had been of iron, all might have escaped with little or no injury.

If iron was introduced for joists, rafters, and beams, they might all be cast hollow, they might all be screwed and pinned together, and have a very light appearance, at the same time possessing much more strength than wood. If the spars, on which the floor is laid, were made tight and laid near each other; and cast with a small projecting edge on each side at the bottom of each spar, so that, when laid down, to form the floor, a flat tile, or thin quarry, would just fit in between two spars; when all the interstices of the floor were filled up with cheap tiles or quarries made on purpose, the floor would be fire proof, and made so at a very little expense; as the spars might be cast light, there being more in number, and would be nearly if not quite as cheap as wood spars; and all the additional expense would be the common flat tiles, which would not be of much extra value, nor give much trouble in the laying; on which fire proof floor, the boards might be laid.

Iron beams and rafters, & fire proof floors.

By the introducing of iron for timber, the danger of fire would be much less to be dreaded; for, if a room took fire, its contents and floor could only be destroyed; and the fire could not easily be communicated from room to room. Indeed I do not see how it is possible for it to extend. The large timbers, that now connect rooms together, would be taken away, which timbers being burnt through, the floor falls, and overwhelms in destruction the rooms and furniture below.

Communication from room to room.

Floors could not fall in, if laid on iron. As only the
VOL. XXIV.—OCT. 1809. K boards

boards on them could be burnt, roofs could not fall in, if the beams, rafters, &c. were iron. In fact, a fire could not make its way and spread, if iron was substituted for the timbers now used in building, and few if any lives would ever be lost, if the staircases were made of iron also.

I am, your obedient servant,

Caroline Street, Aug. 22d, 1809,

B. COOK.

VIII.

On Respiration. By Mr. J. ACTON. In a Letter from the Author.

DEAR SIR,

Ipswich, 22d Aug. 1809.

Respiration a
subject of im-
portance.

Its interruption
always produ-
ces death:

but resuscita-
tion may some-
times be af-
fected.

AGREEABLY to the conclusion of my last letter, I enter now upon respiration. No subject can be more important, more deserving investigation and serious reflection, than that on which animal life so essentially depends. Whether its utility be referred to the medical and chemical philosopher, as enabling him to take more comprehensive views of the cases submitted to his decision, or as generally increasing the sum of human knowledge, it is still the same. The consequences resulting from a thorough insight into this most important function of vitality exceed all calculation. Some other of the animal functions may be arrested by disease, and their action altogether cease for a time, and the animal shall still continue to live; but in the instance under consideration there cannot be a complete interruption with impunity, whether it take place by immersion in water, or in noxious air, or by a ligature tied round the trachea. Cut off by any means the communication between the lungs and atmospheric air, and the animal dies; the obvious effect is instantaneous, and nearly similar. It must be admitted however, that resuscitation by timely interference may frequently be brought about, after animal life has been for some time apparently extinct; but in many instances a few minutes deprivation are sufficient to destroy the vital spark, beyond the possibility of revival. I do not flatter myself with
being

being able to throw much additional light on a subject, which has been already so extensively discussed by men of the highest attainments: the principal end I have in view is merely to endeavour to establish, as in germination, the simple phenomenon of the absorption of oxygen gas by the blood in the lungs, in contradiction to the theory which supposes the emission of solid carbon, and its subsequent union with the oxygen gas of the air to form carbonic acid gas, as stated in my former paper. For this purpose I have confined my experiments to the greatest simplicity, conscious how important it must be to demonstrate this one circumstance beyond any doubt, and thereby afford the means of unerring data for constituting a more distinct theory of respiration: for I believe it is an axiom in natural philosophy, as well as mathematics, that, if the data be founded in error, the conclusions derived from them must be false. In order therefore to establish any doctrine upon a secure foundation, it would appear very desirable in the first place, to endeavour to remove existing doubts, which perhaps cannot be better done than by the institution and arrangement of a certain number of facts, placed in so appropriate and lucid a point of view, as shall be calculated to carry conviction to the inquiring mind. These only ought to be considered as the pillars and support of every rational theory; and the decay and failure of them from after experiment and improvement should be the signal for its vanishing away, to be replaced by more accurate results. For my own part, in pursuing these investigations I put in no claim for novelty, and my direct object is confined within very narrow bounds. I must confess, that my greatest pleasurable feeling arises principally from the contemplation of the probability of future benefit being derived from them by their stimulating others, who have more energy of mind, with better opportunities and advantages, to resume them with additional ardour, so as to carry them to the greatest perfection they may be capable of. Could I be convinced, that any experiment I shall perform, or any sentence flowing from my pen, may have so desirable an effect, my highest ambition would be satisfied, and I should console myself with the reflection, that I have not lived in vain.

Object of the
present paper.

Facts the only
secure foundation of any
doctrine.

Cruelty to animals.

A great deal has been lately said respecting cruelty to animals. It has necessarily occurred, that, in the following experiments, it has been found impossible to avoid putting them to pain. I can only say, that every wanton infliction of it has been studiously avoided. Those animals which are considered as most noxious and insignificant have been preferred for the purpose. And I have no doubt, if the part of natural history treating of the economy of domestic vermin were more diffused and understood, they would be yielded up without reluctance by the most humane and tender hearted for experimental purposes, where the intention is evidently for instruction and improvement, and not to satisfy mere idle curiosity. Mice, for instance, it is well

Mice

increase very rapidly.

When their food begins to be exhausted, they swallow dirt and sand.

This the stomach soon rejects,

and then they prey on each other.

known, under favourable circumstances, such as plenty of food and the absence of their natural enemies, increase with the most astonishing rapidity, the female often producing nine young ones at a single parturition. The consequence is, the numbers sooner or later begin to exceed the means of subsistence. When this first happens, and the food is nearly or quite exhausted, they endeavour to repel the first attacks of hunger by picking up dirt and sand in sufficient quantity to have the mechanical effect of distending the stomach and intestines. But the delicate coats of the stomach will not long bear the repetition of this artificial food, the sharp angles of the particles of sand at length irritate and inflame that organ, weaken its powers, and compel it to reject it altogether. No other resource is then left to the animal, but to try to sustain its existence by feeding on those of its own species it can overcome, thus, impelled by the corroding sensations of hunger to recur to this unnatural but only method left them to satisfy the most voracious appetites. And this will proceed till nearly or quite the whole community be extinct, if no opportunities of emigration, or supply of food, present itself. I make these remarks from actual observation: and I am sure it need not be suggested, that often the individual sufferings of these little animals must be very great in this way, without noticing the length of time they are frequently tormented when in the power of their worst enemy the cat; so that it is merciful to increase the means of

Not so cruelly destroying them. I believe also it will be found that the manner

manner in which they have been treated in these experiments must not be compared in severity to the common mode of exterminating them by poison. It is on the first transient view that humanity shudders at the infliction of pain; could she have patience to listen to adequate reasons for what is done, conviction of utility would often take place of censure. Far, very far be it from me by the least word or action to advocate the cause of cruelty: should I be suspected of so great depravity, I can only say, I feel conscious of deserving no such accusation; it is foreign to my nature; not the poles of the Earth are more distant from each other than is inhumanity in any shape from my genuine feelings. It must however be allowed, and with concern I mention it, that philosophers have sometimes in recording their experiments particularized the most painful operations on animals with an indifference not very characteristic of a tender nature, and sufficient almost to induce a suspicion of a deficiency of the finer traits of sensibility, particularly of that species so masterly portrayed and inculcated in the instructive and pleasing writings of Mr. Pratt.

In the following experiments it will be seen there is a sameness and want of variety bordering upon tediousness, which the simplicity of the fact sought to be demonstrated can alone excuse. I was desirous not to lose any thing for want of repetition; and if by this means a sufficiently strict analogy be apparent in them, the end will be as well answered, as in all probability it would have been by extending them in a more complicated form, which might only have had the effect of rendering the deductions less plain and easy.

I must premise, without farther apology, that in these as well as my former experiments, it has not been possible to avoid the introduction of small quantities of atmospheric air; but it will appear, that, so far from having any tendency to vitiate these results, they become in most instances a farther confirmation of them.

18 Oct. 1808, Temp. 45°, Press. 29.20.

Exp. 1. An accurately graduated jar being filled with quicksilver and inverted, 17.50 cub. inches of atmospheric Mouse kept in atmospheric air

air over mercury 50°.

air were passed up (the air of the laboratory being previously examined and ascertained by the average of several trials at a medium temperature and pressure to be composed of 20 parts oxygen and 80 parts nitrogen). A mouse was put through the mercury into the jar, and suffered to remain 50 minutes. When withdrawn the air was found to have diminished 1·25 c. in. On exposure to lime water, a farther absorption was observed of 100 c. in. The remaining 15·25 c. in. being exposed to liquid sulphuret of potash, 00·46 c. in. were absorbed, leaving a residue of 14·79 c. in., which was nitrogen.

Liquid sulphate of iron impregnated with nitrous gas a better test of oxygen than sulphuretted alkali.

It is evident the accuracy of this experiment may be called in question; for, according to the analysis of the air of the laboratory, the whole diminution should have been 3·50 c. in.; but it was only 2·71 c. in., making a difference of 00·79 c. in., which I attribute to the attempt at operating with the whole quantity of gas, instead of taking an aliquot part of it, which I have since done, and always found to be more easy and true. Neither do I think the sulphuretted alkalis so good and rapid tests of oxygen gas as the liquid sulphate of iron impregnated with nitrous gas.

30 January, 1809. Temp. 44°, P. 28·94.

Mouse killed in oxygen gas.

Exp. 2. Into an inverted Jar in the same manner as the above were passed up 13 cubic inches of oxygen gas nearly pure. A mouse was then conveyed through the mercury into the jar, where it was suffered to remain an hour and a quarter, when it was quite dead, and the gas had in that time diminished 1·00 cubic inch. An accident prevented the farther prosecution of this experiment.

Another.

Exp. 3. At the same time another mouse was placed in a like quantity of oxygen nearly two hours. When taken out it was quite dead. The gas had diminished as before 1·00 cubic inch, and being then examined by lime water, 89·50 per cent disappeared, showing, that the animal had absorbed nearly the whole of the oxygen, and given out a considerable quantity of carbonic acid gas.

19 Feb. Temp. 63°, P. 30·10.

Two mice killed.

Exp. 4. Two mice were suffered to die in one cubic inch of

of atmospheric air over mercury. Upon trying the residue with lime water, 12·90 per cent only were absorbed, evidently showing the oxygen was not all consumed; and consequently the animal dies while yet a portion of oxygen remains; most probably from the combined effect of the carbonic acid gas produced, and the effluvia transpiring from the animal's body, and which from this species in particular is most highly offensive and disgusting.

Exp. 5. The above two mice, when taken out of the jar while yet warm, were passed up another inverted jar containing 4 cubic inches of atmospheric air. After remaining four days no material diminution could be perceived. A portion of the air was then tried with lime water, and 18 per cent absorbed. I did not proceed farther with this experiment; for, being convinced how different must be the chemical action of bodies possessing vitality, and those undergoing decomposition, I did not see the utility of endeavouring to trace any analogy between them: but to satisfy myself what were the aerial products arising from the putrefaction of animal bodies cut off from contact with the surrounding air, I instituted the following experiment:

Exp. 6. Into an inverted jar filled with mercury I passed up a mouse so recently dead as to be quite warm. The next day a little gas had been produced, which continued to increase, and in seven days amounted to about 2·50 cubic inches. A liquor of a pale red colour had oozed from the body, amounting to about 0·15 parts of a cubic inch of a most fetid and disgusting smell. 100 parts of the gas being exposed to lime water, 81 parts disappeared. The remaining 19 parts being submitted to the test for oxygen, 3 parts were absorbed. The residual gas appeared to be nitrogen. If any ammonia had been formed, it must have been contained in the liquor, and the fetor arising from that was so very powerful, as to prevent my distinguishing or entering into any nice examination respecting it; but I have some reason to believe its formation is considerably facilitated by the presence of atmospheric air.

19 Feb. Temp. 63°, P. 30·10.

Exp. 7. A mouse being passed up an inverted jar over quicksilver, Mouse in exhausted in 1 c. inch of atmospheric air.

quicksilver, containing 1.62 cubic inch of oxygen nearly pure, after 50 minutes it had diminished 0.19 of a cubic inch. The gas being then exposed to caustic potash only 0.31 of a cubic inch were absorbed.

14 April, Temp. 46°, P. 29.90.

Another.

Exp. 8. Another was placed in the same situation in 1.10 cubic inch of oxygen gas. When nearly dead it was withdrawn, and the air found to have decreased 0.40 of a cubic inch. The remainder being submitted to caustic potash, 0.30 of a cubic inch more were taken up, leaving a residue of only 0.40 of a cubic inch.

15 April, Temp. 45°, P. 29.30.

Another.

Exp. 9. Another was placed in the same situation for 50 minutes in 2.40 cubic inches of oxygen. When the mouse was in, the scale indicated 3.05. At the expiration of the above time it was reduced to 2.80. The decrease would doubtless have been more, but for the casual introduction of some atmospheric air.

19 April, Temp. 42°, P. 29.80.

Another.

Exp. 10. Another placed in the same situation in 1 cubic inch, or 100 parts of oxygen, 29 parts were absorbed. The remaining 71 parts exposed to lime water lost 20 parts more, leaving only 51 parts.

20 April, Temp. 48°, P. 29.60.

Another.

Exp. 11. A mouse was passed up a jar in the same manner into 3.10 cubic inches of oxygen gas. At the expiration of 50 minutes the air had decreased to 2.30, which being exposed to caustic potash, 1.60 were absorbed.

Several mice suffocated in nitrogen.

Exp. 12. Several mice having been suffocated in 4 cubic inches of nitrogen gas, upon trial with lime water it became turbid, and 4 per cent were absorbed.

Mouse suffocated in hydrogen.

Exp. 13. A mouse having been suffocated in hydrogen gas, on examination by lime water a trace also of carbonic acid gas could be perceived.

24 June,

24 June, Temp. 65°, P. 29.95.

Exp. 14. A mouse being placed in the usual manner in a jar, containing 3 cubic inches of oxygen of the purity of ^{Mouse in oxygen gas.} 98, in 30 minutes it was quite dead. The air had then decreased by observation 1.00 cubic inch. The residuum being treated with lime water, 33.67 per cent disappeared. This was the average of two trials, and 100 parts taken of the remainder, and submitted to impregnated sulphate of iron, 84 per cent were absorbed, making the whole by calculation stand thus.

3.00	cubic inches of oxygen gas	Statement of the gas.
1.00	diminished in respiration	
<hr/>		
2.00		
0.67	absorbed by lime water	
<hr/>		
1.33		
1.11	absorbed by test for oxygen	
<hr/>		
.22	Residue, being nitrogen.	
<hr/>		

At the same time another was passed up into 3.50 parts of oxygen gas, and at the expiration of 40 minutes 0.90 parts were diminished.

10 July, Temp. 62°, P. 29.70.

Exp. 15. In the same manner a mouse was passed up into 1.70 cubic inch of oxygen gas. After it was in, the scale indicated 2.80 cubic inches. When withdrawn in ten minutes, 1.50 cubic inch. 100 parts being then examined with lime water, 21 parts were taken up, and the remaining 79 parts being exposed to the test for oxygen, 67 parts disappeared, leaving a residue of 12 parts, which appeared to be nitrogen. ^{Mouse in oxygen gas.}

11 July, Temp. 60°, P. 29.80.

Exp. 16. Another being passed up into 1.60 cubic inches of oxygen gas, the scale then indicated 2.40 cubic inches. When the mouse was quite dead, it had diminished to 2.05; and after it was taken out to 1.25; showing an absorption of ^{Another.}

of 0.35 cubic inches. 100 parts being exposed to lime water, 46 parts were taken up.

14 July, Temp. 65°, P. 29.93.

Another.

Exp. 17. Another being in the same situation in 1.14 cubic inches of oxygen gas, the scale indicated 1.81. In 20 minutes it had diminished to 1.30, an absorption of 0.51 of a cubic inch having taken place. The jar being by accident upset, it could be proceeded with no farther.

Same Time.

Another.

Exp. 18. Another being placed in 1.30 cubic inch of oxygen gas, by the scale it was then 2.18. In 20 minutes it had diminished to 1.70 cubic inch. 100 parts by lime water were reduced to 32 parts, which, exposed to the test for oxygen, left only 13 parts of nitrogen.

Same Day.

Another.

Exp. 19. A mouse being put into 1.15 cubic inches of oxygen gas in the same way, the scale, after it was in, indicated 2.00 cubic inches. At the expiration of 20 minutes it was reduced to 1.47 cubic inch. 100 parts by lime water were reduced to 43 parts; and these, exposed to the test for oxygen, lost 22 parts, leaving 21 of nitrogen.

Same Day.

Another.

Exp. 20. Another being put into 1.10 cubic inch of oxygen gas, the scale indicated 1.80 cubic inch; and at the end of 20 minutes 1.50, there being an absorption of 0.50 of a cubic inch. Out of 100 parts lime water took up 51 parts: the remaining 49 parts being submitted to the test for oxygen, 31 parts disappeared, leaving 18 parts of nitrogen.

Same Day.

Mouse in atmospheric air.

Exp. 21. Another being placed in 4 cubic inches of atmospheric air, the scale then indicated 4.70. In 20 minutes it had decreased to 4.35 cubic inches. 100 parts being then tried with lime water, 13 parts were taken up; and of the remaining 87 parts exposed to the test for oxygen, 6 parts

6 parts were absorbed ; leaving a residue of 81 parts of nitrogen.

Same Day.

Exp. 22. Another being placed in 4 cubic inches of atmospheric air, the scale then indicated 5 cubic inches. In 20 minutes it had decreased to 4.80 cubic inches. 100 parts being then tried with lime water, 15 parts were taken up ; and of the remaining 85 parts, exposed to the test for oxygen gas, 5 parts were absorbed ; leaving a residue of 80 parts of nitrogen.

From the general complexion of these experiments it must be obvious, that, although for reasons easily to be assigned they are not always to the same extent in the decrease of air from respiration, it is still sufficiently demonstrated, there is an absorption of oxygen in every case more or less. Certainly they must be considerably influenced by the state of the animals at the time of the experiment, some of them being more recently caught, and more healthy than others, as well as by the difference in the capacity of the lungs. The noisome effluvia continually emitted from their bodies by transpiration must have its effect, as it appears in no case can the whole of the oxygen gas be absorbed in respiration. Therefore the carbonic acid gas formed, and these effluvia together, terminate their existence, when there sometimes remains even more oxygen than in common air. When animals die in confined portions of atmospheric air, it is also true a portion of the oxygen remains unconsumed, but in much less proportional quantity. It will be seen in the experiments with oxygen gas nearly or quite pure, a proportion of nitrogen has been left, in some instances more than a fifth of an aliquot part of the whole gas tried, which doubtless must have arisen from the introduction of some atmospheric air, when the animals were passed through the quicksilver into the jars, and I have no doubt a little is given out from the lungs.

Another.
More or less
oxygen absorbed
in every
case by respiration :

but not the
whole.

Less oxygen
left in atmospheric air.

Nitrogen left
after respiration of oxygen.

I should have been content with giving these experiments as they are, and suffering such inferences to be drawn from them as their tendency may warrant, being perfectly satisfied in my own mind the absorption of oxygen is fully proved ;

Oxygen absorbed in respiration,

not converted
into carbonic
acid.

Oxygen ab-
sorbed by the
blood.

proved; particularly from having always observed, when mice are put into this gas, the greatest decrease is always in the first few minutes. And that neither by analogy nor experiment have we any right to assume, that the decrease takes place from the condensation occasioned by the conversion of the oxygen and solid carbon into carbonic acid gas, as supposed by Mr. Ellis. Indeed I do not think I should again have commented on this subject, but by being forcibly struck with the most singular perversion (I dare not say intended) of one of the celebrated Mons. Bichat's experiments quoted from his work, "*Recherches sur la Vie et la Mort*," the original of which I had an opportunity of seeing for a short time only, in support of this new theory, and which was intended and does absolutely go to demonstrate the absorption of the oxygen gas by the blood. It was my intention to have repeated this very interesting experiment; but being about to institute others, having some relation to it, when opportunity will permit, I have preferred delaying it till that time. Thus stands the quotation in Mr. Ellis's work on Germination, &c. p. 128. "Air, says Mr. Bichat, thrown into the vascular system, quickly brings on agitation, convulsions, and death. (P. 179 of Mr. B.'s work). By forcing air through the windpipe into the lungs with a syringe, and confining it there, he has made it to enter into the blood vessels, which immediately brings on agitation and exertion in the animal. And if an artery in the leg or foot be now opened, the blood will spring out frothy and full of bubbles of air. If hydrogen gas has been used, the bubbles may be inflamed, and when this frothy blood has flowed thirty seconds, the actions of life cease, and cannot be again restored, even although fresh air be applied. (P. 303 of Mr. B.'s work)."

I regret I have not now by me Mr. B.'s work, and I have not heard of either that or his *Anatomie Générale* being yet translated. I have however now before me a very copious analysis of it, which will be quite sufficient to enable me to point out the application Mons. Bichat intended by the above passages. But first I hope it will not be deemed improper, if I depart from the subject for a moment. As an enthusiastic

enthusiastic admirer of physiological pursuits, I have experienced the greatest delight in reading an account of Mr. Bichat's labours. I was astonished at the irresistible manner in which his experiments and demonstrations carry conviction to the mind; and I cannot but deeply lament, in common with every lover of science, that so sublime and ardent a genius should be suddenly cut off in the midst of his useful and instructive career. I believe none of his works have yet been translated into English, nor the originals much diffused through this country. I hope however, if not already done, no long period will elapse before so desirable an object is accomplished. His physiological and anatomical writings deserve to be most carefully studied, particularly by those of the medical profession, ere they can be duly appreciated. I am by no means competent to decide upon the merits of performances executed by so extraordinary a man. I can only say, if upon a careful perusal they shall leave upon the minds of others the same strong impressions, that a partial knowledge of them has left upon mine, they can never be obliterated; and I have no doubt of their occasioning so much ardour and discussion in the progress of these pursuits, as will eventually be productive of the most beneficial consequences to mankind, by fixing the structure of medical science upon the immovable basis resulting from the combination of the most liberal and enlightened theories with the most decisive facts and practical experience. Hence shall arise out of the ashes of unstable and departed hypothesis a permanent superstructure, invulnerable to the attacks of mere speculatists, and which, though solid and immovable in itself, shall still admit of being improved and beautified by the labours of present and future artists. I trust I may be excused for thus taking the liberty of paying this trifling tribute of admiration and esteem to the memory of the ever to be lamented Bichat, though he was never known to me but through the medium of his writings. From the sketch of his life, which I have read, I may be confident in stating, that my veneration cannot be misplaced on him as a man, however my ability to understand and value him as a writer may be readily called in question—that he was one of those, who will ever have a
first

first rank among the illustrious dead, will not I believe, be disputed.

Mr. Ellis asserts,

that no oxygen enters into the blood.

But to return to the purpose, for which the above quotation was made. It is necessary, for the illustration of my proposition, I should make a short extract from Mr. Ellis's work. At page 198 he says: "We have endeavoured to prove, that no gasses either exist in the blood, or can be transmitted through the vascular and cellular structure interposed between the air and that fluid in the lungs: consequently no oxygen can enter into the blood, to unite with its supposed carbon; nor, if such union did take place, could the carbonic acid be afterward expelled from that fluid."

Bichat found hydrogen gas enter the blood through the lungs.

Now is it not wonderful, that Mr. Ellis, writing thus, should make the before mentioned extract from Mr. Bichat in proof of it; which, instead of being so, goes directly to contradict it? for in the analysis of his work before me, after stating the injection of hydrogen gas into the lungs, and keeping it there by a ligature on the trachea; and demonstrating its passage into the blood by opening an artery, and presenting a lighted taper to the air bubbles formed on the surface the blood which issues out; he thus continues: "*This affords a proof of the passage of air into the blood* THROUGH THE LUNGS, in ADDITION to that of healthy respiration, &c.

Air injected into the blood vessels kills by its mechanical action.

The deadly effect does not take place in the heart.

Death of the heart the effect

The injection of air into the veins or arteries occasioning the destruction of animal life can be no proof, that oxygen gas is not chemically absorbed by the blood in the lungs in healthy respiration; for in the blood vessels it evidently kills by its mechanical action only. Neither does the deleterious effect take place in the heart, as has been supposed; for Bichat has shown, and indeed I myself have often seen, that the heart beats long after the signs of animal life are extinct. Air injected into the carotid artery has the same effect as in the veins, with the addition of agitating the heart by contact as a mechanical body. And also if injected into the vena portæ, but taking a much longer time before the animal is affected, as the capillary circulation of the liver prevents its arrival so soon at the brain. And hence it has been concluded, that the death of the heart is the effect, and not the

the cause of the death of the brain. But the injection of air into the crural artery never proves mortal, though it occasions a paralysis of the muscles. of that of the brain.

Mr. Ellis could not surely for a moment suppose, that because the absorption of oxygen gas by the blood in the lungs in healthy respiration is contended for, therefore the same must take place with respect to hidrogen, or any other mephitic gas; or else with neither: and yet such is the inference naturally presenting itself upon comparison of the above two quotations. As an attentive observer of the changes ensuing in blood by its conversion from venous to arterial, I am firmly persuaded, it is by chemical affinity alone, and not a mere mechanical absorption, such as would take place with water and oxygen or carbonic acid gas. It is only by pressure that hidrogen gas can enter the blood vessels, for in natural inspirations of that air no such effect can be discovered as that by the lighted taper; and judging from analogy we may conclude the same of the rest of the mephitic airs. Besides, it has been ascertained, that the whole is again thrown out of the lungs unaltered in the next expirations; and, as we have already seen, when oxygen gas is breathed, it is far otherwise. Nonabsorption of mephitic gasses by the blood in the lungs no proof, that it does not absorb oxygen.

The blood, in circulating through the pulmonary vessels, presents itself to the air cells to receive its accustomed supply of oxygen; and when noxious airs are respired, or respiration suspended, being continually disappointed it still flows towards all the organs of the body, and their arteries become filled with black blood, till at length the animal becomes asphixated: that is, the volume of the blood returned by the veins is increased; the venous blood not having the power to stimulate the organs of secretion, their functions remain unperformed; the matter that should be secreted returns therefore with the mass; while the venous blood, circulating in the bronchial arterie produces the same deleterious effect on the lungs, as on the other organs; and from the deficiency of oxygen, which is to the air cells what food is to the stomach, the cells cease to be expanded; and at length, by producing a similar effect on the walls of the heart, so enfeebles its contractions, it cannot surmount the resistance set up by the lungs. And, as Bichat energetically Manner in which death is brought on from a defect of oxygen in the lungs.

Resuscitation.

Oxygen gas a powerful auxiliary of resuscitation-

State of the sanguineous system in mice killed in oxygen,

and of those killed in other gasses.

An atmos-

cally expresses it, when once the black blood, (that is the blood which has absorbed no oxygen) has penetrated the heart's tissue, it is dead to sympathy, as well as direct stimuli. Hence it may be inferred, that, in suspended animation from drowning or otherwise, till this fatal effect takes place upon the heart, it is capable of sympathizing in the excitement of the lungs by inflation: that is, it continues susceptible of the impression or action of the oxygenated blood. This knowledge ought to be a still more powerful inducement with us, to persevere in our efforts to restore our fellow beings when they have been by any means accidentally suffocated: and perhaps one of the most powerful auxiliaries we could make use of for this purpose would be the judicious application of pure oxygen gas for the inflation of the lungs, which must evidently be more effectual than common air, and might always be kept in readiness over water in the usual places appropriated for containing the proper resuscitative apparatus, and highly creditable to the philanthropy of some of the inhabitants is it to say, that in this town there are several.

Upon examining the lungs of the mice which died in oxygen, their vascular substance appeared to be engorged with dark red blood; and on observing the liver I found it to be much lighter coloured than usual, no doubt from deficiency of blood. It would naturally have been expected, that, from the action of the oxygen gas on the blood in the lungs, it would have appeared of a florid colour: but in consequence of the great excitement, occasioned by the rapid absorption of this gas in respiration, to the lungs, intercostals, and diaphragm, the mechanical action of the lungs must have first ceased, and several rounds of circulation must afterward have gone on from the continued action of the heart, which I have sometimes known to beat nearly an hour after the discontinuance of the animal functions.

In those which died in atmospheric air, nitrogen, hydrogen, and carbonic acid gasses, I invariably found the lungs collapsed and empty, and the liver full of blood. The circulation in these cases being more suddenly interrupted, time enough was not allowed to fill the vessels of the lungs.

The effect of oxygen on animals placed in it appears to be

be similar to what occurs when they are exposed in a room of atmospheric air heated many degrees beyond the temperature of the body: the excitement in each case is equally great; and, if it continue, death will be eventually occasioned in both from the same cause, the too rapid absorption of oxygen. In heated air the circulation is proportionably quickened, and a larger surface of blood is of course presented to its influence. And the action by chemical affinity between the blood and the oxygen is no doubt in such circumstances considerably increased. I have sometimes seen dogs sleeping by a large fire excited to such a degree, as at length to respire with great difficulty; the action of the diaphragm has been very violent; and they have in consequence awoken, and been compelled though reluctantly to move.

where highly heated acts like oxygen gas.

But the most deleterious and noxious of all the gasses to animal life is the oxygenized muriatic acid. If it be pure and recent, animals die the instant they are put into it, and the effects upon the thoracic viscera are most dreadful.

Oxygenized muriatic acid most deleterious.

In examining some mice suffocated in it, I found the lungs converted into a dark purplish brown pulp; the heart, auricles, and vessels, become black, dry, and corrugated; the membranes and other parts nearly destroyed; and the blood coagulated into a mass like an electuary. The brain too appeared much inflamed when compared with some that had died in a different manner. But in so small an animal the symptoms cannot be very accurately traced; nor can any other than general observations be made, without considerable difficulty and patience. Death by this means seems to be synchronous; that is, the action of the acid gas is not referrible to any particular organ, but kills the lungs, heart, and brain, all at the same time.

Its effects on mice.

I have often inspected animals suffocated by drowning, and have always found the lungs distended by a quantity of air remaining in the cells; and as this must consist of carbonic acid and nitrogen, might it not be proper, in cases of suspended animation, to draw it out of the lungs by an exhausting syringe, previously to the inflation of them with the oxygen gas?

Drowned animals.

By the politeness of Mr. Stebbing I was permitted to examine
 VOL. XXIV.—OCT. 1808. L Lungs of persons strangled.

examine the lungs of one of the two criminals, who were executed here on the 31st ult. for murder, and delivered to him for dissection. The cells contained a considerable quantity of air; and from the room the lungs appeared to take up in the thorax, I should imagine, they must have been nearly distended to their usual size, as in a living state. Undoubtedly there must be some variation in the appearance of different subjects; and, whether vitality cease suddenly, or in a more gradual manner, the effect on the lungs will in a great measure be determined by it.

Having extended my remarks on this subject farther than I at first intended, I shall defer entering upon vegetation &c. till a future opportunity.

IX.

On the Camera Lucida. In a Letter from Mr. R. B. BATE.

Superiority of
the camera
lucida.

“ The camera lucida is portable in a very small compass ;
“ it represents objects with more brilliancy and distinct-
“ ness than the camera obscura ; and it represents them
“ either singly or in combination with perfect truth and
“ correctness of perspective. What disadvantages has
“ it then to counterbalance these particulars, in which
“ it is evidently superior in a very great degree to the
“ camera obscura ? ”

*See Supplement to Vol. XXIII of Nicholson's
Journal, page 373.*

To Mr. NICHOLSON.

SIR,

Mr. Shel-
drake's state-
ment too un-
favourable to
it.

YOUR correspondent, Mr. Sheldrake, after passing the above encomium on the camera lucida, has put the query which follows, and answered it; but in a manner ill calculated to lead to a fair conclusion upon the subject of his investigation. And, as I have found the camera lucida not only less deficient in the points to which he refers, but to possess many advantages which he appears to have overlooked, I feel induced to state them in a more familiar manner
than

than has hitherto been done; being persuaded, that some of these advantages are not generally known, and likewise influenced by a wish to see justice done to the merit of an invention, which deserves to be better understood, and which is peculiarly admirable for its correctness and simplicity.

It has advantages not generally known.

You very justly remark, that, in using the camera lucida, it is certainly intended "the tracing should be made upon that part of the paper, where the picture and point of the pencil can both be seen coincident; and not that a copy should be taken in the manner described by Mr. Sheldrake." But it is matter of regret, that you should not have enlarged upon the effect of varying the position of the eye; in describing which the ingenious inventor has not been sufficiently minute, as is strongly instanced by the misconception manifested in the case before us.

Method of using it.

The management of the eye has not been described minutely enough.

Mr. Sheldrake evidently confines the camera lucida to the purpose of bringing the reflection of some of the objects upon the upper part of the paper, for the approximated convenience of copying them upon the lower part; instead of placing his pencil among the images themselves, and "rendering them permanent," by tracing their outline at once, as himself states to be done in the camera obscura. As Mr. Sheldrake seems sensible of the advantage of moving his eye to the right and left, it is the more extraordinary, that he should confine himself to that motion, when the transverse motion of the eye is the most obviously important.

Mr. Sheldrake's method of using the instrument erroneous.

In copying a landscape the instrument is to be fixed upon a steady table or board, on which a sheet of paper is stretched, and the prism brought over the middle of it: the open face of the prism is to be placed opposite the centre of the view; the black eye piece, or stop, being in a horizontal position, is to be moved till the lucid edge of the prism intersects the eye hole. The eye should now be brought close to this opening, and, upon looking through it vertically towards the paper, a perfect copy of the view will appear reflected upon it, and the reflected images will be large in proportion to the elevation of the prism. The eye hole should now be drawn farther off the prism, so as to

Proper method.

leave a representation of the object barely distinct, for the more complete command of the pencil.

Management of the eye.

The whole apparatus remaining stationary, it will be found, that, by moving the head so as to carry the eye farther over the prism, and looking inwards, the view will be continued upon the lower part of the paper; and by drawing the eye off towards the edge of the prism, and looking the contrary way, the view will be continued upwards: thus

Field of view.

the reflection of every object comprised within an angle of 45° in height and depth will in succession be distinctly seen; and by a diagonal inclination of the eye towards the right and left a horizontal compass of the landscape equal to an angle of 80° may also be obtained, and few will be dissatisfied with this field of view.

Method of obtaining a clear sight of the image and pencil at the same time.

The pencil may now be employed in following the outline of the images, and, if their brightness should any where impede distinct vision of the former at the point of coincidence, a slight motion of the eye towards the edge of the prism will obtain it, and vice versa when the image is not sufficiently distinct. It may not be amiss to recommend generally the near edge of the prism to be kept in a line with the pencil and the image; for which purpose it will obviously be necessary to move the head in a direction opposite to the motion of the pencil, that the eye may follow it, and keep it in contact with the lower edge of the picture, or rather, the edge of that part of the reflection which is at the instant visible.

Directions for copying a near or very tall object.

When the instrument is used in copying a near or very tall object it may occasionally be found, that, in following the image towards the upper part of the paper, the eye will be confused with the original reflection, which is coloured and inverted; it will then be necessary to enlarge the field of view, by turning the prism upon its pin, slightly inclining its face upwards, and depressing the near edge of the stop; which may be done without inconvenience, for, while the pin is strictly confined to a motion in that direction*, the

* To ensure the confinement of this motion to the vertical direction, a small clamp for the top of the outer stem will be found useful; this may be tightened as soon as the elevation for the prism is determined on, and will answer to prevent the inner stem from sliding down or turning round.

images will not be in the least shifted from their places on the paper, which is a great advantage belonging to this instrument.

A much more important advantage peculiar to the camera lucida is the essential benefit a young artist may derive from a limited use of it. For instance, to have the outline of one or two objects, situate near the middle of the view, as reflected by the prism: and afterwards to look directly at the view itself, using the upper edge of the prism as a guide for the point of observation. His eye and judgment may then be exercised in determining, by this outline, the relative magnitudes and distances of the remaining objects; occasionally referring to the reflection of them in the prism for their true situations in comparison with those his judgment has assigned them: and these corrections, attentively observed, seem capable of affording the most valuable aid in cultivating a delicacy of discrimination. The finished artist will also find a great economy of time, upon extensive and complicated subjects in particular, by using the instrument in determining the situations of so many points as he may deem important; and which the camera lucida is allowed to give "with perfect truth and correctness of perspective."

Peculiar advantage of the camera lucida to the student,

and to the artist.

Though hitherto omitted, it is proper to notice the frequent impediments to an extent of view, arising from the projection of near objects; parts of the head-dress in particular are sometimes unsuspected obstructions, and the brim of the hat the most formidable of all.

View obstructed by the brim of the hat &c.

Dr. Wollaston has briefly adverted to the method of enlarging a drawing, or delineating minute objects as magnified; by bringing the eye piece to a vertical position and looking directly at the object through the eye-hole and the lens, which must be turned up likewise to the same position; the paper and pencil then appear reflected in front of the object, more or less distinctly, according to the quantity of prism exposed to the pupil: and a delineation of the object may be obtained large in proportion to the magnifying power of the glass and the surface of the paper occupied. To this I beg to add, that a compound microscope may be used in the same manner, but more conveniently with the horizontal

An object may be drawn magnified by the camera lucida.

A compound microscope may be used

in the same manner.

horizontal position of the eye hole, by bringing the microscope to the same position, and the face of the prism close to its first eye glass. A telescope may likewise be employed; having previously removed the head or cover, the face of the prism must be brought into contact with the eye glass, to which it serves as a diagonal eye piece: a distant object is then approximated, appears reflected upon the paper as before, and may be delineated in a manner at once pleasing, novel, and correct.

The camera lucida superior to every other contrivance.

These combined advantages, and above all, the truth of the reflected image under every circumstance, give the camera lucida a decided superiority over all other known contrivances for the same purpose. And if the hints I have offered should enable Mr. Sheldrake, or any of your readers, to derive farther gratification in the use of the instrument than they have hitherto received, or call to notice any unexpected purpose, to which it may be applied, I shall be happy in having contributed, however poorly, to that gratification.

I am, Sir,

Poultry, 12th Sept. 1809.

Your very obedient servant,

R. B. BATE.

X.

An Account of some Experiments, performed with a View to ascertain the most advantageous Method of constructing a Voltaic Apparatus, for the Purposes of Chemical Research.
By JOHN GEORGE CHILDREN, Esq. F. R. S*.

Best construction of a voltaic battery desirable.

THE late interesting discoveries by Mr. Davy having shown the high importance of the voltaic battery, as an instrument of chemical analysis, it became a desirable object to ascertain that mode of constructing it, by which the greatest effect may be produced, with the least waste of power and expense.

Battery after

For this purpose, I made a battery, on the new method,

* Philos. Transact. for 1809, p. 92.

with

with plates of copper and zinc, connected together by leaden straps, soldered on the top of each pair of plates; which are twenty in number, and each plate four feet high, by two feet wide: the sum of all the surfaces being 92160 square inches, exclusive of the single plate at each end of the battery. The trough is made of wood, with wooden partitions well covered with cement, to render them perfectly tight, so that no water can flow from one cell to another. The battery was charged with a mixture of three parts fuming nitrous, and one part sulphuric acid, diluted with thirty parts of water, and the quantity used was 120 gallons.

the new method.
Plates of 4 feet by 2.
Surface 92160 square inches.

In the presence, and with the kind assistance of Messrs. Davy, Allen, and Pepys, the following experiments were made.

Experiment 1. Eighteen inches of platina wire, of $\frac{1}{30}$ of an inch diameter, were completely fused in about twenty seconds. Its effects.

Exp. 2. Three feet of the same wire were heated to a bright red, visible by strong day-light.

Exp. 3. Four feet of the same wire were rendered very hot; but not perceptibly red by day-light. In the dark, it would probably have appeared red throughout.

Exp. 4. Charcoal burnt with intense brilliancy.

Exp. 5. On iron wire, of about $\frac{1}{10}$ th of an inch diameter, the effect was strikingly feeble. It barely fused ten inches, and had not power to ignite three feet.

Exp. 6. Imperfect conductors were next submitted to the action of the battery, and barytes, mixed with the red oxide of mercury, and made into a paste with pipe-clay and water, was placed in the circuit; but neither on this, nor on any other similar substance, was the slightest effect produced.

Exp. 7. The gold leaves of the electrometer were not affected.

Exp. 8. When the cuticle was dry, no shock was given by this battery, and even though the skin was wet, it was scarcely perceptible.

Before I offer any observations on the inferences to be drawn from these experiments, I shall mention some others, performed, for the sake of comparison with the foregoing, with

with an apparatus very different in size and number of plates from the one just described.

Couronne de
tasses; plates
2 inches
square;
surface 3200
square inches.

This second battery was precisely the *couronne des tasses* of Sig. Volta, consisting of two hundred pairs of plates, each about two inches square, placed in half pint pots of common queen's ware, and made active by some of the liquor used in exciting the large battery, to which was added a fresh portion of sulphuric acid, equal to about a quarter of a pint to a gallon.

Its effects.

To state as shortly as possible the effects produced by this battery:

Experiment 1. It decomposed potash and barytes readily.

Exp. 2. It produced the metallization of ammonia with great facility.

Exp. 3. It ignited charcoal vividly.

Exp. 4. It caused considerable divergence of the gold leaves of the electrometer.

Exp. 5. It gave a vivid spark, after being in action three hours. At the expiration of twenty-four hours, it retained sufficient power to metallize ammonia, and continued, with gradually decreasing energy, to produce the same effect, till the end of forty hours, when it seemed *nearly* exhausted.

Intensity of the
electricity in-
creases with
the number, &
quantity with
the extent of
the series.

From the results of the foregoing experiments, which, though simple and not numerous, I trust, are satisfactory; we see Mr. Davy's theory of the mode of action of the voltaic battery confirmed: he says (in his Paper on some chemical agencies of electricity, sect. 9, after having shown the effect of induction to increase the electricity of the opposite plates) "the *intensity*, increases with the *number*, and the *quantity* with the *extent* of the series*"

This proved by
the preceding
experiments,
on perfect con-
ductors,

That this is so, the effects produced on the platina and iron wires, in the first and fifth experiments with the large battery, and the subsequent experiments on imperfect conductors with the small apparatus, sufficiently prove. The platina wire being a perfect conductor, and not liable to be oxidated, presents no obstacle to the free passage of the electricities through it, which, from the immense quantities given out from so large a surface, evolve, on their mutual an-

* Journal, vol. XIX, p. 55.

niliation, heat sufficient to raise the temperature of the platina to the point of fusion.

With the iron wire, of $\frac{7}{16}$ th of an inch diameter, the effect is very different, which is explained by the low state of and imperfect conductors. the intensity of the electricity) sufficiently proved by its not causing any divergence of the gold leaves of the electrometer); which being opposed in its passage by the thin coat of oxide, formed on the iron wire, at the moment the circuit is completed, a very small portion only of it is transmitted through the wire. To the same want of intensity is to be attributed the total inability of the large battery to decompose the barytes, and its general weak action on bodies which are not perfect conductors. The small battery, on the contrary, exerts great power on imperfect conductors, decomposing them readily, although its whole surface is more than thirty times less than that of the great battery; but in point of number of plates, it consists of nearly ten times as many as the large one.

The long continued action of the small battery proves the utility of having the cells of sufficient capacity to hold a large quantity of liquor, by which much trouble of emptying and filling the troughs is avoided, and the action kept up, without intermission, for a long space of time, a circumstance, in many experiments, of material consequence. Importance of having the cells sufficiently capacious, Beside this advantage, *with very large combinations*, a certain distance between each pair of plates is *absolutely necessary*, to prevent spontaneous discharges, which will otherwise ensue, accompanied with vivid flashes of electric light, and some distance between each pair of plates. as I have experienced, with a battery of 1250 four-inch plates, on the new construction.

And here I beg leave to mention an experiment, which, though not directly in point, cannot be considered as foreign to the subject of this paper. It has been urged, as one proof of the nonidentity of the common electricity, and that given out by the Voltaic apparatus, that in the latter there is no striking distance. That objection, however, must cease. I took a small receiver, open at one end; through perforations in the opposite sides of which were placed two wires, with platina points, well polished: one was fixed by cement to the glass, the other was movable, by Argument for the dissimilarity of common and Voltaic electricity done away. means

means of a fine screw, through a collar of leathers, and the distance between the points was ascertained by a small micrometer attached. This receiver was inverted over well dried potash over mercury, and suffered to stand a couple of days, to deprive the air it contained, as thoroughly as possible, of moisture. The 1250 plates being excited precisely to the same degree as the great battery, mentioned in the beginning of this communication; and the little receiver placed in the circuit, I ascertained its striking distance to be $\frac{1}{10}$ of an inch. That I might be certain, that the air in the apparatus had not become a conductor by increase of temperature, I repeated the experiment several times with fresh cool air, and always with the same result; but perhaps it will be objected, that the striking distance was so small, as not to afford a satisfactory refutation of the argument alluded to, when it is considered to how very great a distance, comparatively, the spark of the common electrical machine

Striking distance .02 of an inch, with 1250 plates.

This distance might be increased.

Another proof of their identity.

Numerous combination fused but little platina.

Effect of the apparatus in the compound ratio of the number & size.

can pass through air. The answer to this is obvious: increase the number of the plates, and the striking distance will increase; for we see throughout, the intensity proportioned to the number, and it probably may be carried to such extent, as even to pass through a thicker plate of air, than the common spark. The great similarity of the appearance of the electric light of this battery in vacuo, and that of the common machine, might also be urged as an additional proof of the identity of their nature.

The effect of this large combination on imperfect conductors was, as may be supposed, very great; but of the same platina wire, of which the four-feet plates fused eighteen inches, this battery melted but half an inch, though, had the effect been in the ratio of their surfaces, it should have fused nearly fourteen inches.

The absolute effect of a Voltaic apparatus, therefore, seems to be in the compound ratio of the number, and size of the plates: the intensity of the electricity being as the former, the quantity given out as the latter; consequently regard must be had, in its construction, to the purposes for which it is designed. For experiments on perfect conductors, very large plates are to be preferred, a small number of which will probably be sufficient; but where the resistance

of imperfect conductors is to be overcome, the combination must be great, but the size of the plates may be small; but if quantity and intensity be both required, then a large number of large plates will be necessary. For general purposes, four inches square will be found to be the most convenient size. 4 inches square a convenient size.

Of the two methods usually employed, that of having the copper and zinc plates joined together only in one point, and movable, is much better than the old plan of soldering them together, through the whole surface, and cementing them into the troughs: as, by the new construction, the apparatus can be more easily cleaned and repaired, and a double quantity of surface is obtained. For the partitions in the troughs, glass seems the substance best adapted to secure a perfect insulation; but the best of all, will be troughs made entirely of Wedgwood's ware, an idea, I believe, first suggested by Dr. Babington. Plates joined together in one point only preferable.
Troughs of Wedgwood's ware best.

XI.

Report of a Memoir of Mr. HASSENFRATZ, respecting the Alterations, that the Light of the Sun undergoes in traversing the Atmosphere. By Mr. HAUY.*

THE class of physical and mathematical sciences having directed Mr. Laplace and me to examine a paper of Mr. Hassenfratz on the changes that the solar light undergoes in passing through the atmosphere, we shall proceed to give an account of it.

The light of the sun being composed of an infinite number of rays of different tints, the union of which forms white, we should always see it white, if it came to us in the state in which it is emitted from that body. But in passing through the atmosphere it frequently undergoes alterations, that change its appearance, so that there are circumstances in which it appears to us with its natural whiteness, and The sun would always appear white, if its rays were not affected in passing through the atmosphere.

* Journal de Physique, vol. LXVI, p. 256.

others in which it appears yellow, orange coloured, or red. According to Mr. Hassenfratz these different effects depend in general on the state of the atmosphere, the difference of the latitude, and the elevation above the sea. As to the ultimate cause of these phenomena, it was natural to ascribe them, as Mr. Hassenfratz does, to the suppression of a part of the rays of the solar light in its passage through the atmosphere. Newton has already announced this property of transparent mediums, to stop certain of the rays that enter them, letting the rest pass on; and this celebrated philosopher even remarks, that they are frequently absorbed one after another at different distances from the surface at which the light entered; and he quotes for example the various tints exhibited in succession by a coloured fluid in a conical glass, which is placed between the eye and the light, and raised so as to have the thickness traversed by the visual ray continually increasing.

Object of the author to determine the kind and quantity of rays intercepted.

Now Mr. Hassenfratz proposes to determine the number and kinds of rays, the suppression of which occasions the various tints, that alter the primitive whiteness of the solar light. The means he has employed are founded on a rule given by Newton, to determine the colour produced by a given mixture of rays of different kinds taken from those that compose the solar spectrum. It follows from this, that, if we can know the sorts of rays that the atmosphere takes away from the solar light, we shall know by necessary consequence the colour produced by the mixture of the species remaining, and we may judge whether this colour be the same as that, under which the disk of the sun presents itself. Here we must observe, that the mixture producing a given colour may be more or less compounded, because a colour does not change, at least with regard to its species, by the addition of parts of the spectrum situate on each side at equal distances from the point considered as the centre of this colour. For instance, if we add to the green its two contiguous colours, blue and yellow, we still have green; and the mixture will remain green, if we farther add indigo and orange, one of which is contiguous to the blue and the other to the yellow. Nothing but direct experiment therefore can indicate the species of rays absorbed in their passage,

sage, when the disk of the sun appears yellow, orange, or red.

Mr. Hassenfratz concluded, that the observation of the solar spectrum produced by the refraction of the prism would lead him to his object, because, the spectrum being necessarily incomplete from the absence of the rays intercepted in their passage, the determination of the deficiencies in the spectrum would indicate these; and it might afterward be ascertained whether the colour resulting from a mixture of the rays remaining would be the same as that of the solar disk.

This to be done by observation of the spectrum.

Mr. Hassenfratz cites several results of the experiments he made under the various circumstances of which we are speaking. Thus on the 13th of January, 1807, having observed the spectrum at ten o'clock in the morning, he found the violet wanting, with part of the indigo. Now according to Newton's rule, if the violet be suppressed, with a certain portion of the indigo, the remaining colours are those, which, by their mixture, produce yellow: and the disk of the sun appeared of the latter colour. As a necessary consequence, the yellow of the spectrum was deeper than ordinary. The same day at noon, the sun was white, and the spectrum then had its whole extent. But at four in the evening the violet had disappeared anew, with a greater quantity of indigo, so that the sun appeared of a deeper yellow than at ten in the morning. Lastly, at a quarter after four the spectrum was shortened on the same side, and in consequence the solar disk inclined to orange.

Observations made.

Mr. Hassenfratz presented to the class several coloured drawings of the solar spectrum, such as he observed them in circumstances where it had lost more or less of its length. The drawings were made at the moment of the experiment by Mr. Gerard, at the Polytechnic School.

Drawings of the spectrum taken.

The author adds, that he has sometimes remarked the effects of the subtraction of several rays in rainbows seen at different hours of the day, which have exhibited varieties in the number or extent of the coloured arcs.

Subtraction of rays from the rainbow.

The experiments are interesting in themselves, because they serve to explain in a natural and satisfactory manner some phenomena, on which we had not any thing precise.

They

They deserve the attention of the natural philosopher also from the influence these phenomena have in experiments relating to the decomposition of light.

SCIENTIFIC NEWS.

Cultivation of
fruit trees.

MR. van Mons, of the Institutes of France and Holland, is publishing a "Theoretical and Practical System of Fructiculture, or Instructions for the Work of the Nursery and Fruitgarden in the Order of the Months." The extensive correspondence of the author having brought him acquainted with all the improvements lately made in this branch of science by a great number of persons distinguished for their education and talents, who have withdrawn from the fatigues of war or the toils of politics, or who, grieving at public and private calamities, or chagrined at the ingratitude and injustice of mankind, have retired to forget their sorrows in the quiet enjoyment of their gardens, he has conceived he should be rendering a service to many, by making them more generally known. The work, which commenced in January last, and will finish with December, is on the principle of a gardener's calendar, and will include every thing relating to the culture of fruit. It will give in detail the whole management of fruit trees in the nursery and in the garden, not from books however, but from the author's own experience, and the communications of his friends.

Middlesex Hospital.

Medical Lectures.

Dr. SATTERLEY's Course of Clinical Instruction at the Middlesex Hospital will begin the first week in November: the attendance on the patients will be continued daily, and Lectures will be given once a week, or oftener, when it may be necessary, at eleven o'clock. **Mr. CARTWRIGHT**, Assistant Surgeon to the Hospital, will undertake such occasional demonstrations of morbid anatomy, as may be required for the illustration of the respective cases. The objects of the Course will also be extended to such remarkable peculiarities

peculiarities in the diseases of children, as may occur in the Foundling Hospital.

Dr. YOUNG will begin his Elementary Lectures on Chemistry, Physiology, the Practice of Physic, and the Materia Medica, about the middle of December: he will deliver them on Mondays, Wednesdays, and Fridays, at 7 o'clock in the evening, throughout the season.

Chemical and
Medical Lec-
tures.

St. George's Hospital, and George Street, Hanover Square.

On Saturday, October the 7th, a Course of Lectures on Physic and Chemistry will recommence in George street, at the usual morning hours, viz. the Therapeutics at eight; the Practice of Physic at half after eight; and the Chemistry at a quarter after nine. By GEORGE PEARSON, M.D. F.R.S. Senior Physician to St. George's Hospital, of the College of Physicians, &c.

Medical and
Chemical Lec-
tures.

Clinical Lectures are given as usual on the patients in St. George's Hospital, every Saturday morning, at nine o'clock.

Dr. SQUIRE will begin a Course of Lectures on the Theory and Practice of Midwifery, and the Diseases of Women and Children, on the 3d of October, at his house, Ely Place, Holborn.

Obstetrical
Lectures.

The notice received from Dublin, and given in our Journal, No. 103, p. 239, that Professor Davy intended to give a Course of Lectures on Galvanism in that city, was erroneous; it being incompatible with the situation and duties of that gentleman to lecture in any other place than in the Royal Institution, where the usual Courses must be in progress at the very time mentioned in the said notice.

To CORRESPONDENTS.

Mr. Rootsey's paper is obliged to be set aside, from my printers being unable to procure from the letter-founders the requisite types.

Mr. Singer's communication will appear next month.

J. S. of Hatton-garden, is deferred on account of the engraving. In the mean time I should be glad, if he would favour me with his address.

METEOROLOGICAL JOURNAL,

For SEPTEMBER, 1800,

Kept by ROBERT BANCKS, Mathematical Instrument Maker,
in the STRAND, LONDON.

AUG. Day of	THERMOMETER.				BAROME- TER, 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.		Day.	Night.
26	58	60	62	52	29.80	Rain	Cloudy
27	57	61	63	54	29.75	Ditto	Fair
28	59	61	67	55	30.00	Fair	Ditto
29	62	65	71	62	30.08	Ditto	Ditto
30	66	68	73	62	29.89	Rain	Ditto *
31	63	62	66	58	29.85	Cloudy	Ditto
SEPT. 1	60	62	64	57	29.80	Ditto	Ditto
2	60	64	69	60	29.68	Rain	Ditto
3	64	65	71	59	29.60	Ditto	Ditto
4	64	64	69	58	29.62	Ditto †	Cloudy
5	63	62	68	58	29.54	Ditto	Ditto
6	64	62	68	57	29.52	Ditto	Ditto
7	62	60	67	54	29.27	Ditto	Ditto
8	57	59	65	52	29.34	Ditto	Ditto
9	59	61	67	53	29.58	Ditto	Fair
10	60	60	64	50	29.67	Rain	Ditto
11	57	56	64	48	29.70	Ditto	Ditto
12	56	56	64	47	29.78	Fair	Ditto
13	54	56	64	53	29.85	Rain	Cloudy
14	58	58	62	54	29.68	Ditto	Fair
15	60	58	64	49	29.98	Fair	Ditto
16	57	59	63	49	30.14	Rain	Rain
17	55	58	64	52	30.00	Fair	Fair
18	58	56	64	51	29.90	Rain	Ditto
19	55	55	64	50	29.72	Fair	Ditto
20	55	56	65	49	29.47	Rain	Ditto
21	55	56	65	52	29.64	Fair	Cloudy ‡
22	58	62	66	54	29.60	Rain	Fair
23	61	56	65	50	29.64	Ditto	Ditto
24	56	50	58	48	29.85	Ditto	Ditto
25	53	48	53	40	29.70	Ditto §	Ditto
26	43	50	53	47	30.00	Fair	Cloudy

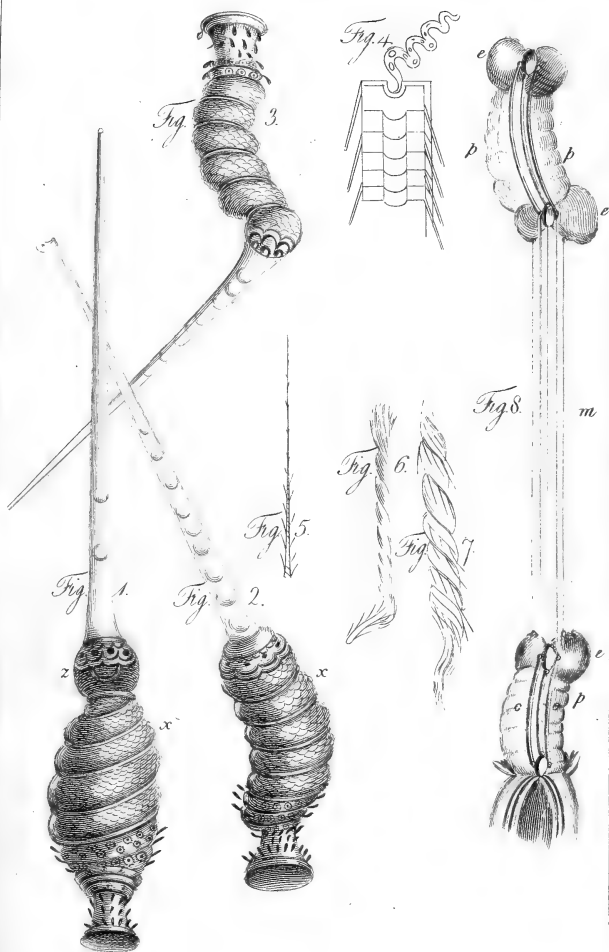
* Lightning in the East.

† Thunder, at 1 P.M.

‡ Hazy moon.

§ The maximum of the thermometer at 9 A.M. gradually subsiding during the day. The morning cold and rainy.

|| Hazy moon.



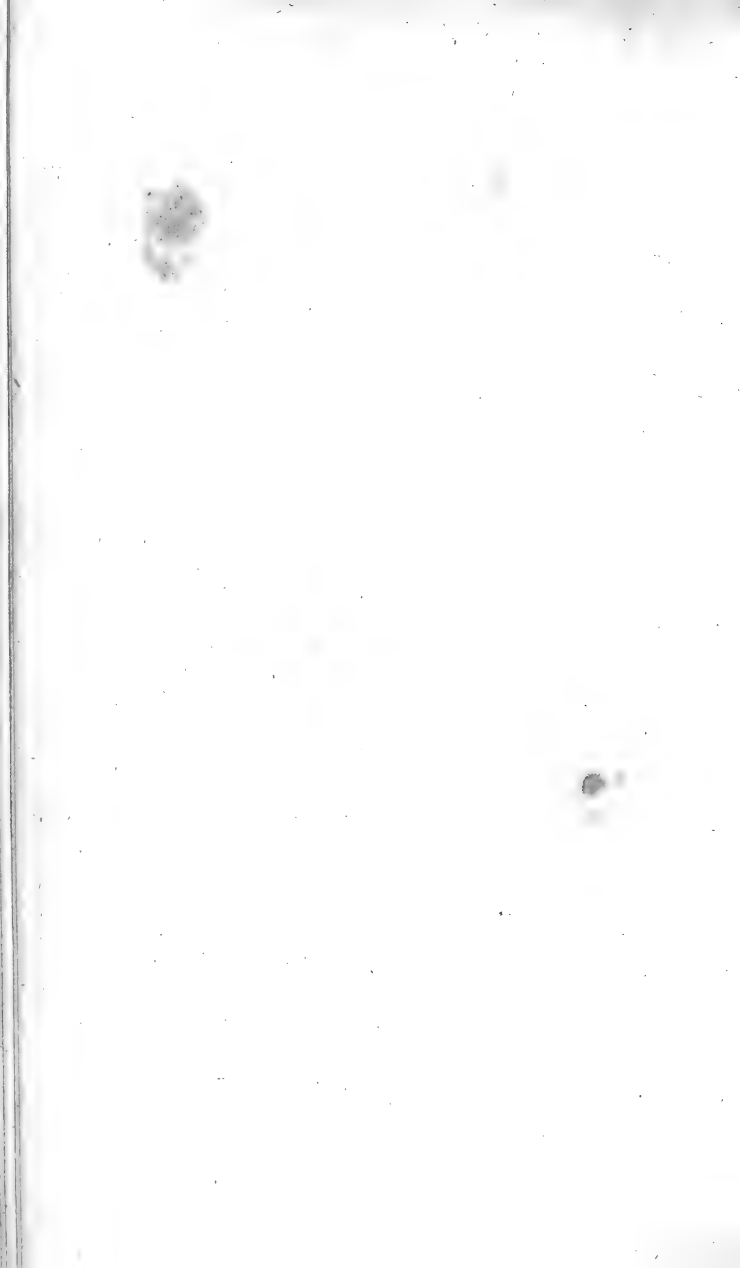




Fig. 2.



Fig. 5.



Fig. 4.

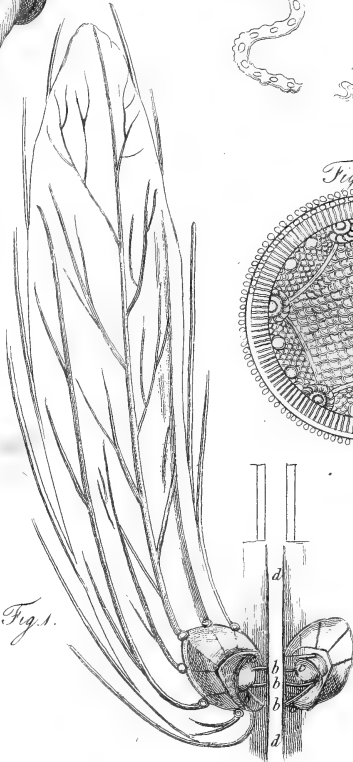


Fig. 1.

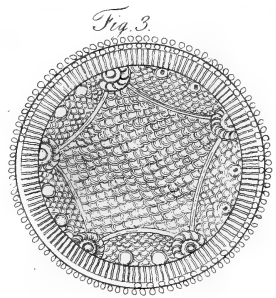
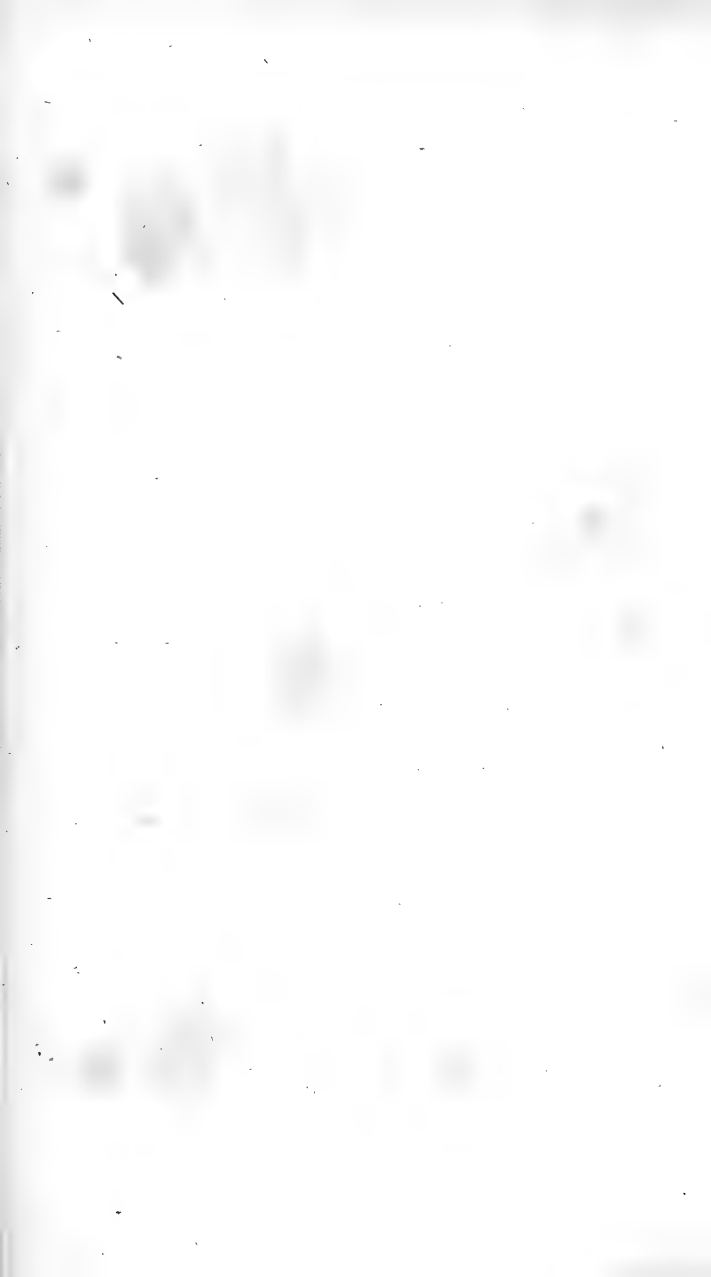
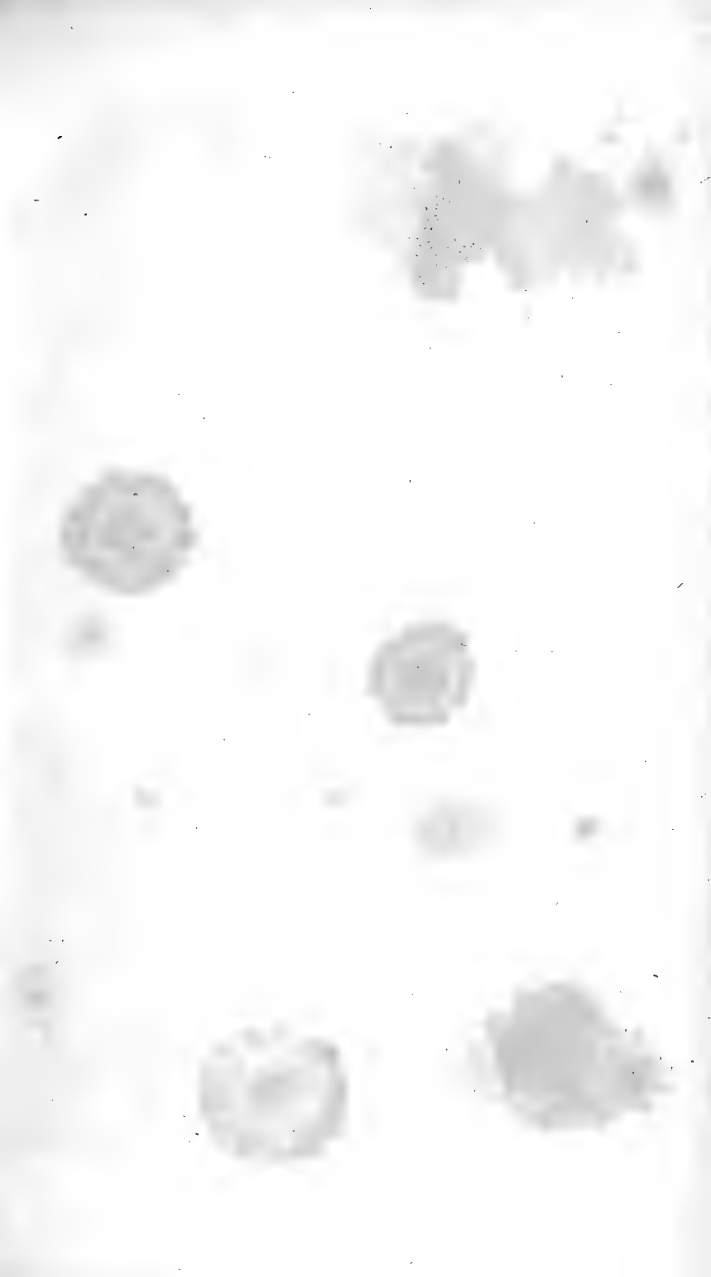


Fig. 3.





Id
Luminous Effects on Snow & Ice
by J. H. M. C. C. C.



Vol. 1. No. 1. 1857. 17 1/2 x 11 1/2

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1809.

ARTICLE I.

Account of some luminous Meteors seen during a Thunder Storm. In a letter from JAMES STAVELEY, Esq.

To Mr. NICHOLSON,

SIR,

RETIRING rather late to bed last night, and throwing up the window to admire the beauty of the lightning, I was struck with the appearance of the sky, the grandeur and singularity of which I never remember to have seen equalled. The time was about half past one o'clock. Considering that facts of this kind are at all times acceptable to the meteorologist, and that this may perhaps serve to elucidate some of the mysteries of that science yet unfathomed, if you have no better account of the phenomenon, may I offer this for a place in your valuable Journal?

The whole surface of the heavens seemed covered with one unbroken mass of black pitchy cloud, in which the very vivid flashes of lightning, that almost instantaneously succeeded each other, showed no break; and from which,

Appearances
of the sky in a
thunder storm

described.

Vol. XXIV—No. 108.—Nov. 1809. M but

but from inferior regions, they did not seem to issue: Over, or rather (to speak more properly) below this apparent surface, were spread light and flocky clouds, broken into large fleeces, white and apparently luminous throughout. I looked round that I might find whence proceeded the light that illuminated them; for they seemed as summer clouds in a bright sun, and as the clouds have appeared to-day. I could perceive no light. Every other part of the hemisphere was totally dark.

White clouds apparently full of specks of light.

A luminous meteor of larger size.

Looking fixedly at them, I fancied, that they seemed full of little dazzling and dancing specks of light, that sometimes shone as stars peeping through a misty cloud. Some of these increased gradually, and as gradually died away. One in particular became more and more distinctly visible, and increased in size, till it reached the brilliancy and magnitude of Venus, as she shines in a clear evening: and yet, there seemed no *body* of the light. At first I thought it must be some star; and it was with difficulty, that I renounced the idea. But such it could not have been: for, when these clouds had passed away, and when the intensity of the black masses above became diminished, when they seemed only concealed by a dark and thick haze, none of them became visible. To be certain that the motion, that, I fancied, I observed it to have, did not proceed from the motion of the cloud, and was not deceitfully produced to me, from the swimming and indistinctness of vision necessarily occasioned in my eyes by the quick and vivid flashes of lightning, that encircled the whole horizon, I brought the meteor to a bearing with the window frame, and by that means distinctly ascertained its movement, and, that it was with considerable rapidity. I observed it *coast*, if I may use the expression, round the edge of that mass in which it appeared, and, having again become stationary, diminish from its full splendor till it disappeared. Its duration must have been of minutes.

Another meteor of the same kind.

After a short interval I had an opportunity of observing another of these meteors in a similar cloud, though that at a considerable distance; and of which, though it behaved much as the former had done, I was not able so distinctly to mark the motion.

During

During this time, these, and during the space of half an hour at least, similar clouds, were full of these little luminous innumerable points, which, playing incessantly, gave them an appearance similar to that, which is exhibited in a clear sky by the galaxy.

Several clouds resembling the milky way.

I have already said, that, when these had passed away, and the pitchy clouds also, which moved in the same direction, though not so rapidly, I could discern no stars whatever, and I took no small pains to spy out any, as they might have furnished me with a solution of the phenomenon. There was no flash of *lightning broke* from these clouds, but they emitted much light of a pale phosphoric colour, and such seemed the kind of light, that formed the body of the meteors. These clouds were at a very considerable distance beneath the higher stratum, and at no great elevation in the atmosphere, and though, after the interval of an hour, some of the most vivid flashes proceeded from this point in the heavens, yet do I conceive no connection between them and the clouds; as the latter had clean passed away, in an easterly direction, or with a few points north. Another thing I must mention, that as they tended to a greater distance, their brilliancy gradually diminished.

No star visible.

No lightning from the luminous clouds.

Along with this account I have enclosed a sketch of the phenomena, wherein, though guilty of an anachronism, having in the same moment of time shown the two meteors, I may be pardoned, as I have tolerably preserved the relative bearings of distances, and, as nearly as in such a sketch I could, the respective forms of the masses of cloud. The course of the first I have marked by making it luminous throughout; and note, that its first appearance was to the eastward, which in this sketch being the left hand, the position will be best seen when the sketch is held in the position of observation, above the head. It is on a very proportionally small scale, as at least 35 degrees are included within it, and the spots noted for the meteors proportionally as large, as was the halo that seemed to surround them. I am afraid to have dilated too much; yet, not seeing where I can curtail the description, leave for you, Sir, to lop off any superfluous matter.

Explanation of the plate.

No rain at the time. There was no rain at the time of observation.

I am, Sir,

Hatton Garden,

Your humble servant,

11 Aug. 1809.

J. S.

P. S. As these meteors increased in size, they seemed to descend, and had much of that semblance, which the phantasmagorical spectres have, as they seem to approach the spectator.

II.

On Aerial Navigation. By Sir GEORGE CAYLEY, Bart.

SIR,

Brompton, Sept. 6, 1809.

A man raising himself into the air by mechanical means.

I Observed in your Journal for last month, that a watch-maker at Vienna, of the name of Degen, has succeeded in raising himself in the air by mechanical means. I waited to receive your present number, in expectation of seeing some farther account of this experiment, before I commenced transcribing the following essay upon aerial navigation, from a number of memoranda which I have made at various times upon this subject. I am induced to request your publication of this essay, because I conceive, that, in stating the fundamental principles of this art, together with a considerable number of facts and practical observations, that have arisen in the course of much attention to this subject, I may be expediting the attainment of an object, that will in time be found of great importance to mankind; so much so, that a new æra in society will commence, from the moment that aerial navigation is familiarly realized.

The principles may be reduced to practice.

It appears to me, and I am more confirmed by the success of the ingenious Mr. Degen, that nothing more is necessary, in order to bring the following principles into common practical use, than the endeavours of skilful artificers, who may vary the means of execution, till those most convenient are attained.

Since

Since the days of Bishop Wilkins the scheme of flying by artificial wings has been much ridiculed; and indeed the idea of attaching wings to the arms of a man is ridiculous enough, as the pectoral muscles of a bird occupy more than two thirds of its whole muscular strength, whereas in man the muscles, that could operate upon wings thus attached, would probably not exceed one tenth of his whole mass. There is no proof that, weight for weight, a man is comparatively weaker than a bird; it is therefore probable, if he can be made to exert his whole strength advantageously upon a light surface similarly proportioned to his weight as that of the wing to the bird, that he would fly like the bird, and the ascent of Mr. Degen is a sufficient proof of the truth of this statement.

Artificial wings attached to the arms cannot answer.

But the whole strength of a man applied to a machine may.

The flight of a strong man by great muscular exertion, though a curious and interesting circumstance, in as much as it will probably be the first means of ascertaining this power, and supplying the basis whereon to improve it, would be of little use. I feel perfectly confident, however, that this noble art will soon be brought home to man's general convenience, and that we shall be able to transport ourselves and families, and their goods and chattels, more securely by air than by water, and with a velocity of from 20 to 100 miles per hour.

Confident expectation of its accomplishment.

To produce this effect, it is only necessary to have a first mover, which will generate more power in a given time, in proportion to its weight, than the animal system of muscles.

First mover requisite.

The consumption of coal in a Boulton and Watt's steam engine is only about $5\frac{1}{2}$ lbs. per hour for the power of one horse. The heat produced by the combustion of this portion of inflammable matter is the sole cause of the power generated; but it is applied through the intervention of a weight of water expanded into steam, and a still greater weight of cold water to condense it again. The engine itself likewise must be massy enough to resist the whole external pressure of the atmosphere, and therefore is not applicable to the purpose proposed. Steam engines have lately been made to operate by expansion only, and those might be constructed so as to be light enough for this purpose, provided

Steam engine.

provided the usual plan of a large boiler be given up, and the principle of injecting a proper charge of water into a mass of tubes, forming the cavity for the fire, be adopted in lieu of it. The strength of vessels to resist internal pressure being inversely as their diameters, very slight metallic tubes would be abundantly strong, whereas a large boiler must be of great substance to resist a strong pressure. The following estimate will show the probable weight of such an engine with its charge for one hour.

	lb.
The engine itself from 90 to	100
Weight of inflamed cinders in a cavity presenting about 4 feet surface of tube	25
Supply of coal for one hour	6
Water for ditto, allowing steam of one atmosphere to be $\frac{1}{1500}$ the specific gravity of water	32
	<hr/> 163

This statement
merely an ap-
proximation.

I do not propose this statement in any other light than as a rude approximation to truth, for as the steam is operating under the disadvantage of atmospheric pressure, it must be raised to a higher temperature than in Messrs. Boulton and Watt's engine; and this will require more fuel; but if it take twice as much, still the engine would be sufficiently light, for it would be exerting a force equal to raising 550 lb. one foot high per second, which is equivalent to the labour of six men, whereas the whole weight does not much exceed that of one man.

Another first
mover

It may seem superfluous to inquire farther relative to first movers for aerial navigation; but lightness is of so much value in this instance, that it is proper to notice the probability that exists of using the expansion of air by the sudden combustion of inflammable powders or fluids with great advantage. The French have lately shown the great power produced by igniting inflammable powders in close vessels; and several years ago an engine was made to work in this country in a similar manner, by the inflammation of spirit of tar. I am not acquainted with the name of the person who invented and obtained a patent for this engine, but from some minutes with which I was favoured by Mr.

William

William Chapman, civil engineer in Newcastle, I find that 30 drops of the oil of tar raised eight hundred weight to the height of 23 inches; hence a one horse power may consume from 10 to 12 pounds per hour, and the engine itself less weighty, need not exceed 50 pounds weight. I am informed by Mr. Chapman, that this engine was exhibited in a working state to Mr. Rennie, Mr. Edmund Cartwright, and several other gentlemen, capable of appreciating its powers; but that it was given up in consequence of the expense attending its consumption being about 8 times greater than that of a steam engine of the same force. but more expensive.

Probably a much cheaper engine of this sort might be produced by a gas-light apparatus, and by firing the inflammable air generated, with a due portion of common air, under a piston. Combustion of inflammable air. Upon some of these principles it is perfectly clear, that force can be obtained by a much lighter apparatus than the muscles of animals or birds, and therefore in such proportion may aerial vehicles be loaded with inactive matter. Even the expansion steam engine doing the work of six men, and only weighing equal to one, will as readily raise five men into the air, as Mr. Degen can elevate himself by his own exertions; but by increasing the magnitude of the engine 10, 50, or 500 men may equally well be conveyed; and convenience alone, regulated by the strength and size of materials, will point out the limit for the size of vessels in aerial navigation.

Having rendered the accomplishment of this object probable upon the general view of the subject, I shall proceed to point out the principles of the art itself. Principles of the art. For the sake of perspicuity I shall, in the first instance, analyze the most simple action of the wing in birds, although it necessarily supposes many previous steps. Flight of a bird. When large birds, that have a considerable extent of wing compared with their weight, have acquired their full velocity, it may frequently be observed, that they extend their wings, and without waving them, continue to skim for some time in a horizontal path. Fig. 1, Pl. V, represents a bird in this act.

Let $a b$ be a section of the plane of both wings opposing the horizontal current of the air (created by its own motion) which may be represented by the line $c d$, and is the measure

sure of the velocity of the bird. The angle bdc can be increased at the will of the bird, and to preserve a perfectly horizontal path, without the wing being waved, must continually be increased in a complete ratio, (useless at present to enter into) till the motion is stopped altogether; but at one given time the position of the wings may be truly represented by the angle bdc . Draw de perpendicular to the plane of the wings, produce the line ed as far as required, and from the point e , assumed at pleasure in the line de , let fall ef perpendicular to df . Then de will represent the whole force of the air under the wing; which being resolved into the two forces ef and fd , the former represents the force that sustains the weight of the bird, the latter the retarding force by which the velocity of the motion, producing the current cd , will continually be diminished. ef is always a known quantity, being equal to the weight of the bird, and hence fd is also known, as it will always bear the same proportion to the weight of the bird, as the sine of the angle bde bears to its cosine the angles def , and bdc , being equal. In addition to the retarding force thus received is the direct resistance, which the bulk of the bird opposes to the current. This is a matter to be entered into separately from the principle now under consideration; and for the present may be wholly neglected, under the supposition of its being balanced by a force precisely equal and opposite to itself.

Some practical observations.

Problem.

Experiments on the resistance of the air.

Before it is possible to apply this basis of the principle of flying in birds to the purposes of aerial navigation, it will be necessary to encumber it with a few practical observations. The whole problem is confined within these limits, viz. To make a surface support a given weight by the application of power to the resistance of air. Magnitude is the first question respecting the surface. Many experiments have been made upon the direct resistance of air, by Mr. Robins, Mr. Rouse, Mr. Edgeworth, Mr. Smeaton, and others. The result of Mr. Smeaton's experiments and observations was, that a surface of a square foot met with a resistance of one pound, when it travelled perpendicularly to itself through air at a velocity of 21 feet per second. I have tried many experiments upon a large scale to ascertain

tain this point. The instrument was similar to that used by Mr. Robins, but the surface used was larger, being an exact square foot, moving round upon an arm about five feet long, and turned by weights over a pulley. The time was measured by a stop watch, and the distance travelled over in each experiment was 600 feet. I shall for the present only give the result of many carefully repeated experiments, which is, that a velocity of 11.538 feet per second generated a resistance of 4 ounces; and that a velocity of 17.16 feet per second gave 8 ounces resistance. This delicate instrument would have been strained by the additional weight necessary to have tried the velocity generating a pressure of one pound per square foot; but if the resistance be taken to vary as the square of the velocity, the former will give the velocity necessary for this purpose at 23.1 feet, the latter 24.28 per second. I shall therefore take 23.6 feet as somewhat approaching the truth.

Having ascertained this point, had our tables of angular resistance been complete, the size of the surface necessary for any given weight would easily have been determined. Our tables of angular resistance imperfect. Theory, which gives the resistance of a surface opposed to the same current in different angles, to be as the squares of the sine of the angle of incidence, is of no use in this case; as it appears from the experiments of the French Academy, that in acute angles, the resistance varies much more nearly in the direct ratio of the sines, than as the squares of the sines of the angles of incidence. The flight of birds will prove to an attentive observer, that, with a concave wing Concave wing of a bird. apparently parallel to the horizontal path of the bird, the same support, and of course resistance, is obtained. And hence I am inclined to suspect, that, under extremely acute angles, with concave surfaces, the resistance is nearly similar in them all. I conceive the operation may be of a different nature from what takes place in larger angles, and may partake more of the principle of pressure exhibited in the instrument known by the name of the hydrostatic paradox, a slender filament of the current is constantly received under the anterior edge of the surface, and directed upward into the cavity, by the filament above it, in being obliged to mount along the convexity of the surface, having created

created a slight vacuity immediately behind the point of separation. The fluid accumulated thus within the cavity has to make its escape at the posterior edge of the surface, where it is directed considerably downward; and therefore has to overcome and displace a portion of the direct current passing with its full velocity immediately below it; hence whatever elasticity this effort requires operates upon the whole concavity of the surface, excepting a small portion of the anterior edge. This may or may not be the true theory, but it appears to me to be the most probable account of a phenomenon, which the flight of birds proves to exist.

Experiments
of the French
Academy.

Six degrees was the most acute angle, the resistance of which was determined by the valuable experiments of the French Academy; and it gave $\frac{4}{10}$ of the resistance, which the same surface would have received from the same current when perpendicular to itself. Hence then a superficial foot, forming an angle of six degrees with the horizon, would, if carried forward horizontally (as a bird in the act of skimming) with a velocity of 23.6 feet per second, receive a pressure of $\frac{4}{10}$ of a pound perpendicular to itself. And, if we allow the resistance to increase as the square of the velocity, at 27.3 feet per second it would receive a pressure of one pound. I have weighed and measured the surface of a great many birds, but at present shall select the common rook (*corvus frugilegus*) because its surface and weight are as nearly as possible in the ratio of a superficial foot to a pound. The flight of this bird, during any part of which they can skim at pleasure, is (from an average of many observations) about 34.5 feet per second. The concavity of the wing may account for the greater resistance here received, than the experiments upon plain surfaces would indicate. I am convinced, that the angle made use of in the crow's wing is much more acute than six degrees: but in the observations, that will be grounded upon these data, I may safely state, that every foot of such curved surface, as will be used in aerial navigation, will receive a resistance of one pound, perpendicular to itself, when carried through the air in an angle of six degrees with the

Flight of the
rook.

the line of its path, at a velocity of about 34 or 35 feet per second.

Let ab , fig. 2, represent such a surface or sail made of thin cloth, and containing about 200 square feet (if of a square form the side will be a little more than 14 feet); and the whole of a firm texture. Let the weight of the man and the machine be 200 pounds. Then if a current of wind blew in the direction cd , with a velocity of 35 feet per second, at the same time that a cord represented by cd would sustain a tension of 21 pounds, the machine would be suspended in the air, or at least be within a few ounces of it (falling short of such support only in the ratio of the sine of the angle of 94 degrees compared with radius; to balance which defect, suppose a little ballast to be thrown out) for the line de represents a force of 200 pounds, which, as before, being resolved into df and fe , the former will represent the resistance in the direction of the current, and the latter that which sustains the weight of the machine. It is perfectly indifferent whether the wind blow against the plane, or the plane be driven with an equal velocity against the air. Hence, if this machine were pulled along by a cord cd , with a tension of about 21 pounds, at a velocity of 35 feet per second, it would be suspended in a horizontal path; and if in lieu of this cord any other propelling power were generated in this direction, with a like intensity, a similar effect would be produced. If therefore the waft of surfaces advantageously moved, by any force generated within the machine, took place to the extent required, aerial navigation would be accomplished. As the acuteness of the angle between the plane and current increases, the propelling power required is less and less. The principle is similar to that of the inclined plane, in which theoretically one pound may be made to sustain all but an infinite quantity; for in this case, if the magnitude of the surface be increased ad infinitum, the angle with the current may be diminished, and consequently the propelling force, in the same ratio. In practice, the extra resistance of the car and other parts of the machine, which consume a considerable portion of power, will regulate the limits to which this principle, which is the true basis of aerial navigation, can be carried; and the

Applied to the theory of aerial navigation.

the perfect ease with which some birds are suspended in long horizontal flights, without one waft of their wings, encourages the idea, that a slight power only is necessary.

Farther observations promised.

Experiments have been made on a tolerably large scale.

As there are many other considerations relative to the practical introduction of this machine, which would occupy too much space for any one number of your valuable Journal, I propose, with your approbation, to furnish these in your subsequent numbers; taking this opportunity to observe, that perfect steadiness, safety, and steerage, I have long since accomplished upon a considerable scale of magnitude; and that I am engaged in making some farther experiments upon a machine I constructed last summer, large enough for aerial navigation, but which I have not had an opportunity to try the effect of, excepting as to its proper balance and security. It was very beautiful to see this noble white *bird* sail majestically from the top of a hill to any given point of the plane below it, according to the set of its rudder, merely by its own weight, descending in an angle of about 18 degrees with the horizon. The exertions of an individual, with other avocations, are extremely inadequate to the progress, which this valuable subject requires. Every man acquainted with experiments upon a large scale well knows how leisurely fact follows theory, if ever so well founded. I do therefore hope, that what I have said, and have still to offer, will induce others to give their attention to this subject; and that England may not be backward in rivalling the continent in a more worthy contest than that of arms.

Simple machine rising in the air by mechanical means.

As it may be an amusement to some of your readers to see a machine rise in the air by mechanical means, I will conclude my present communication by describing an instrument of this kind, which any one can construct at the expense of ten minutes labour.

a and *b*, fig. 3, are two corks, into each of which are inserted four wing feathers from any bird, so as to be slightly inclined like the sails of a windmill, but in opposite directions in each set. A round shaft is fixed in the cork *a*, which ends in a sharp point. At the upper part of the cork *b* is fixed a whalebone bow, having a small pivot hole in its centre, to receive the point of the shaft. The bow is then

then to be strung equally on each side to the upper portion of the shaft, and the little machine is completed. Wind up the string by turning the flyers different ways, so that the spring of the bow may unwind them with their anterior edges ascending; then place the cork with the bow attached to it upon a table, and with a finger on the upper cork press strong enough to prevent the string from unwinding, and taking it away suddenly, the instrument will rise to the ceiling. This was the first experiment I made upon this subject in the year 1796. If in lieu of these small feathers large planes, containing together 200 square feet, were similarly placed, or in any other more convenient position, and were turned by a man, or first mover of adequate power, a similar effect would be the consequence, and for the mere purpose of ascent this is perhaps the best apparatus; but speed is the great object of this invention, and this requires a different structure.

P. S. In lieu of applying the continued action of the inclined plane by means of the rotative motion of flyers, the same principle may be made use of by the alternate motion of surfaces backward and forward; and although the scanty description hitherto published of Mr. Degen's apparatus will scarcely justify any conclusion upon the subject; yet as the principle above described must be the basis of every engine for aerial navigation by mechanical means, I conceive, that the method adopted by him has been nearly as follows. Let A and B, fig. 4, be two surfaces or parachutes, supported upon the long shafts C and D, which are fixed to the ends of the connecting beam E, by hinges. At E, let there be a convenient seat for the aeronaut, and before him a cross bar turning upon a pivot in its centre, which being connected with the shafts of the parachutes by the rods F and G, will enable him to work them alternately backward and forward, as represented by the dotted lines. If the upright shafts be elastic, or have a hinge to give way a little near their tops, the weight and resistance of the parachutes will incline them so, as to make a small angle with the direction of their motion, and hence the machine rises.

Method supposed to be employed by Mr. Degen.

A slight

A slight heeling of the parachutes toward one side, or an alteration in the position of the weight, may enable the aeronaut to steer such an apparatus tolerably well; but many better constructions may be formed, for combining the requisites of speed, convenience, and steering. It is a great point gained, when the first experiments demonstrate the practicability of an art; and Mr. Degen, by whatever means he has effected this purpose, deserves much credit for his ingenuity.

III.

On Electro-Chemical Experiments. By Mr. G. J. SINGER.

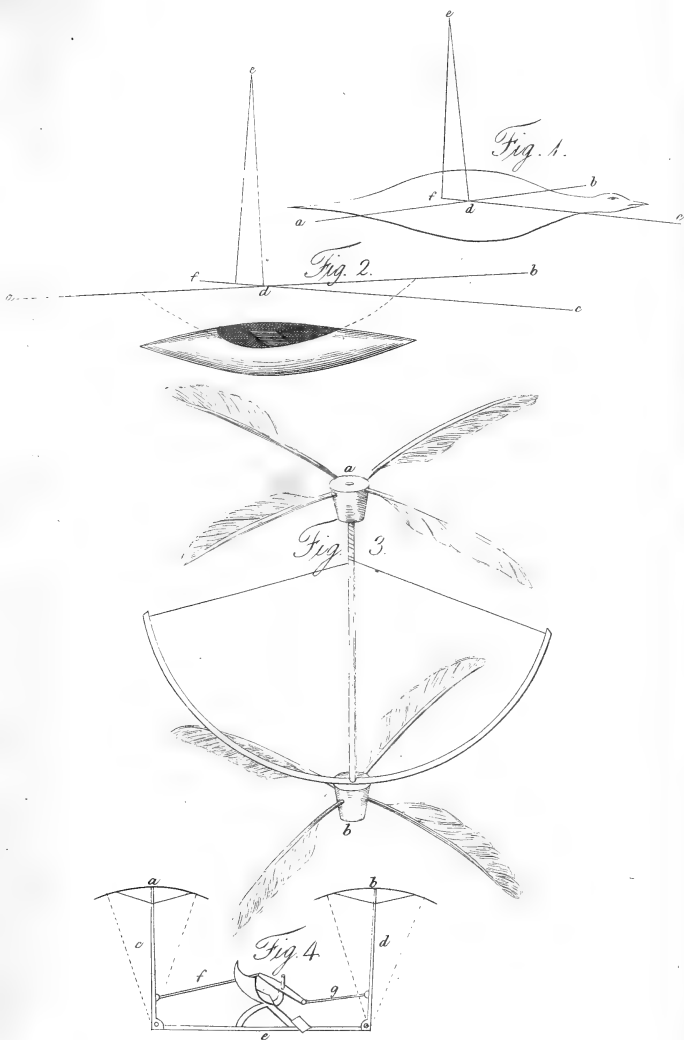
Mr. Davy's experiments not generally repeated.

THE important increase of chemical knowledge, which has attended the recent successful application of electrical powers to the improvement of analysis, cannot be well appreciated without a repetition of the experiments; but this has not been hitherto by any means generally attempted, although a considerable time has elapsed since the publication of Mr. Davy's first researches. The consequences of this delay are most prejudicial to the real interests of science, as the observations, which have been published in this country on the labours of that assiduous chemist, have been consequently supported by nothing but hypothetical reasoning, or loose conjecture; and have been therefore rather calculated to impede than promote the progress of discovery.

Supposed to require great powers,

but these not necessary with due precautions.

From the want of a popular exposition of the facts already obtained, and of the mode of conducting similar researches, an idea has prevailed very generally, that much difficulty attends the repetition of the new experiments; and that in most instances the aid of a powerful Voltaic battery is required. This I have found is not by any means the case, most of the experiments may be performed with very moderate powers, when the requisite precautions are observed. As a want of attention to these circumstances has been in most instances the cause of failure, many experimenters



experimenters having employed very considerable power without effect, it may be useful to describe that peculiar attention, which has been proved by experience most calculated to ensure success.

From a series of experiments made for the express purpose of ascertaining the best mode of employing the Voltaic battery, which I shall at a future period lay before the public, I have found, that the most usual is by far the worst that can be adopted when the instrument is intended for experiments of decomposition; this operation requiring the continued action of a power of nearly uniform intensity, a circumstance that rarely occurs in the ordinary mode of charging. By far the greater number of experimenters estimate the acting power of their instruments by the quantity of wire a given number of plates will fuse; and consider them most advantageously excited, when they fuse the greatest length. To attain this object, if the battery is not of great extent, a strong acid mixture is employed. This produces violent action for a short time, but which gradually decreases, and in a very limited period ceases altogether. The power thus excited, which I call the wire-melting power, is by no means desirable but for the performance of brilliant experiments; the most extensive and interesting class of chemical compounds being either partial conductors, or nonconductors, on which this action will be found less efficacious than a more moderate intensity.

The most active wire-melting power I have yet excited was by a mixture of one part strong nitrous acid, and ten parts water, with the addition of a very minute portion of muriatic acid; but from some observations I have recently made, I am induced to believe this mixture should never be employed in an apparatus used for general experiments.

Three similar batteries were charged with equal proportions of the different acids, that charged with nitric acid fused the greatest portion of wire, that with sulphuric acid the next in quantity, and that with muriatic acid the least: their action on imperfect conductors was nearly similar. At the end of 14 hours they were again tried; the battery charged with nitric acid had completely lost its wire-melting power, as had also that charged with sulphuric acid, and

Best mode of applying the battery for decomposition.

Most effective power for melting wire.

Comparative powers of the three mineral acids.

and neither of them exerted more than a feeble action on imperfect conductors; but the battery charged with muriatic acid, to my great surprise, melted two thirds of the length of wire it had melted in the first instance, and appeared to decompose water with equal rapidity. The three batteries were suffered to remain. At the end of two days the first two had totally lost their acting power, but the last still melted one third of the original length of wire, and continued to melt wire till the fourth day. Its action on imperfect conductors was still evident after six days, when the experiments were discontinued. In all these experiments the plates were lifted out of the acid during the intervals. It was a long time before I could procure an equal continuance of action from the batteries which had first been employed with sulphuric and nitric acids; their powers appeared to be in a measure exhausted, and their action was comparatively feeble, but by persevering in the use of the muriatic acid, I at length brought them to an equal uniformity of action.

Proportion of acid to water.

The quality of the acid to be preferred was clearly proved by these experiments; but it was still necessary to determine the requisite proportion in which it should be employed. For the decomposition of potash this circumstance requires particular attention, as the strength of the mixture should vary with the extent of the apparatus. For any power not exceeding 200 plates of 4 inches, the proportions may be from 8 to 10 ounces of muriatic acid for every gallon of water. But if 300, 400, or any greater number be employed, or their size increased, the quantity of acid should be proportionally diminished, or the heat produced will destroy the metallic globules at the moment of their production.

Naphtha decomposed faster than potash.

In the first experiments I made on this substance, the operation was performed under naphtha, but in this way I found the naphtha was decomposed more rapidly than the potash, and the quantity of carbon liberated embarrassed the result. I now always operate in the open air, and use

Silver preferable to platina.

conductors of silver, which I find preferable to platina. A flat silver plate or spoon is connected with the negative (the copper) surface of the battery. On this I place a small piece

piece of potash, not moistened, and make a communication to its upper surface by a silver wire from the positive surface. In the space of about a minute, or less, metallic globules appear near the negative surfaces. (Some of these inflame, but for the most part they become covered with a crust of potash, which defends them from the farther action of the air). As soon as these globules appear, they should be attentively watched, and the instant they cease to grow larger, removed on the point of a silver knife, and plunged into a watch glass filled with naphtha; or if the experiment is intended merely to show their production, they may be immersed in water as they are removed, when each globule will produce a vivid inflammation.

When the circumstances I have mentioned are strictly attended to, the phenomena are usually as I have now described; but it sometimes happens, that no globules appear: in this case, the communication should be still continued for five or ten minutes, when the potash being taken from the spoon, the side which was in contact with it will be found studded with metallic globules, which may be removed as before directed. Sometimes no globules appear.

By following this mode of operation, I soon found, that a much lower power than I had at all suspected was adequate to the production of a distinct result. With a glass-partitioned battery of 50 pairs of 4 inch plates, the metallic base was produced in sufficient quantity to evince its principal properties with considerable ease. This result induced me to try, if it might not be effected by a lower power; and I have actually found, that, by carefully conducting the process, very distinct globules may be produced by a battery of 50 plates, of only 3 inches diameter, which battery has also the disadvantage of having been much corroded by former operations. The metallization of the alkaline earths and of ammonia by amalgamation with mercury may be also effected by the last described battery; so that even this small power, properly directed, is sufficient to afford a satisfactory illustration of the principal phenomena for the individual gratification of the chemical inquirer. The decomposition effected by a low power.

The transfer of acid and alkali may be very readily shown by an apparatus of this size. The most striking mode of Transfer of acid and alkali.

illustrating this action is the following. To a pint of water add two or three drops of sulphuric acid, and infuse in it as many minced leaves of red cabbage as it will cover. In a day or two, the water will be tinged of a fine red colour. Decant the liquor, and preserve it in a bottle closely stoppered. When the experiment is to be performed, a portion of the red tincture is to be neutralized, by carefully adding a few drops of ammonia, till it assumes a blue colour. Two watch glasses connected by a moistened fibre of cotton, or bibulous paper, are to be filled with this red fluid, and placed in the circuit by connecting one of them with the negative, and the other with the positive wire of the battery. In a short time, this *alkali* will be attracted by the *negative* wire, and the fluid which surrounds it will consequently assume a *green* colour; while the *positive* wire, attracting the *acid*, converts the fluid which surrounds it to a fine *red*. In about half an hour the transfer will be complete, the fluid in the negative cup being of a beautiful green, and that in the positive of a bright red. If the situation of the wires are now reversed, so that the cup which was positive may become negative, and that which was negative assume a positive state, the colours will again change; the *green* will first become *blue*, and then *red*; and the *red*, after first returning to its original *blue*, will become *green*. This alternate transfer, which may be several times repeated with one charge, I have frequently produced by a trough of only 30 pairs of plates of 2 inches square.

Apology for
minuteness.

I have been rather particular (perhaps it may be thought too much so) in the account I have given of these experiments, but I do not write for the instruction of the experienced chemist, and I am inclined to think the tyro will thank me for having attempted to diminish the difficulties of his pursuit. I am at present engaged in the extension of these experiments, and in the prosecution of others connected with the same inquiry, and intend shortly to publish an elementary work on the subject, in which I shall attempt the arrangement of a systematic series of familiar experiments, in illustration of the several phenomena.

3, Princes Street, Cavendish Square,
Sept. 21, 1809.

IV.

Extract of a Letter Mr. J. B. VAN MONS to Mr. SUE, on different subjects relating to Galvanism and Electricity.

ON repeating the experiments of Pacchiani on the pretended decomposition of water into muriatic acid, I satisfied myself, that the galvanic current is a conductor of heat likewise; by interposing between the plates pieces of pasteboard moistened with oximuriatic acid, nitric acid, or oximuriates, nitrates, &c.; which, during their action on the metal of the pile, setting caloric free, the fluid experimented on, or through which the current passes, is heated considerably.

Galvanic current a conductor of heat.

The substances transferred by the action of the pile are partly resolved into their ultimate elements. This resolution does not appear to be effected by means of chemical affinity, or an attraction of composition, but in consequence of the different degree in which these elements are conductible by the galvanic current; which conductibility is measured by the rapidity of the transmission, and the distance beyond the point of the two currents, the positive and the negative, at which it takes place. So that we cannot measure the degree of chemical affinity of a body by that of its decomposability by the pile; but a substance not being decomposable by this apparatus affords a strong presumption, that it is not decomposable by other substances. Substances have not been sufficiently subjected to the immediate action of the pile, or to the effects of the plates on the substance used for impregnating the fluid, with a view to effect their decomposition. Few decomposable substances

Substances carried away by its current, and their elements separated by being acted on with more or less velocity.

would escape being decomposed: even the carbonates are then resolved into their ultimate elements, or set free carbon. When I say carbonates, I speak only of the carbonate of ammonia, for the others appear only to have their base separated from the acid. It would seem as if the activity of the pile were satisfied with the first effect of decomposition it exerts, and that the decomposition of the carbonic acid in the carbonate of ammonia is the effect of a second

This not connected with chemical affinity.

Almost all substances decomposable by the pile.

Carbonate of ammonia.

The galvanic current easily vaporizes substances.

Its effect.

Decomposition of water.

Long continued activity of some piles.

Numerous experiments of the author.

Water not decomposed into muriatic acid.

Source of the acid.

Other acids produced.

dary action, determined by the equal decomposability of the acid and the base; the free acid being too fugacious to be reached by the action of the pile. The facility with which the galvanic current vaporizes in some measure the most fixed substances, as earths, the fixed alkalis, metals, &c., and causes them to circulate with it, is astonishing. The current must intimately dissolve, or strongly divide, the substances it transfers; since after this transference these substances crystallize, as in the beautiful experiments of Brugnatelli, and in experiments similar to those of that illustrious Italian chemist and natural philosopher, which I have performed with earths and alkalis. When the decomposition of water is effected without a perceptible separation of gas, as in the last experiment mentioned by Pacchiani and others, the two gasses follow the galvanic current along the wire with different velocities, and separate only in succession on the body of the pile itself. Most of the substances, that are decomposed by the immediate action of the plates, do not quit the current till their return to that plate or element of the pile, from which they set out; and there they are deposited, recombined, or extricated. To this effect is owing the long continued activity of piles, in which the interposed substance is of a nature to be transferred without being dissipated. I have subjected to the immediate action of the plates all known substances, both solid and liquid, and aeriform in a state of composition; and I have obtained results as extraordinary with respect to the influence of the pile on these substances, as of these substances on the action of the pile. They form a body of facts, from which I have yet deduced but few consequences; but the first moment of leisure I have from my extensive occupations, I shall arrange them, and lay them before the Institute.

I cannot conceive how some persons persist in admitting the decomposition of water into muriatic acid by the pile, while the experiments I have inserted in my Chemical and Physical Journal evidently demonstrate, that, in all cases where this acid is obtained, it comes from a muriate, with which the interposed pasteboards are moistened. This is so true, that, if a solution of some other salt with an indecomposable

possible acid, as a borate or a fluuate, be used instead of a muriate for moistening the disks, the acid transferred corresponds with that of the salt employed. The salts with decomposable acids, as nitrates, sulphates, phosphates, acetates, &c., are in part decomposed by the combined effect of the action of the pile, and of the attraction of the metallic plates for their oxygen. This action appears to strengthen the action of the pile, at the moment when it takes place, if the moistening salt be an oximuriate, or a nitrate, the oxygen of which gives out caloric on entering into a more solid combination. But if it be a salt, the acid of which has what Brugnatelli calls an *oxygenizable* radical, the energy is not very different. This appears to prove, that it is owing to the caloric separated from the oxygen, which, on entering into the galvanic current, is partly transformed into electric fluid. However, I have fancied I have observed a difference of effect here, that is somewhat singular, and shows the great influence of disposing affinity: this is, that the energy of the pile, in respect to what is called its charge, is increased only when the substance subjected to its action is to be decomposed, and that it is almost the same as with other salts, when it is to be composed. We know, that in the first case, namely when principles are to be separated by the direct action of the electric fluid, this fluid, without altering its nature, enters into combination with the principle separated, which it converts into a gas: while in the second case, or when it determines the union of principles that have been separated, it is transformed into heat, simply to raise the temperature: hence the caloric separated from the oxygen will circulate either as electric fluid, or as heat, according as the effect to be produced disposes it to assume or retain the one or other of these modifications. It is not however a determining attraction, that produces the elevation of temperature, of which I spoke in the commencement of my letter. The chemical action then does not heighten the activity of the pile, except as far as this action is accompanied with an evolution of heat.

But no decomposable acid.

The evolution of oxygen strengthens the action of the pile.

Influence of disposing affinity.

Chemical action increases that of the pile only when heat is evolved. The energy of the pile diminished by the

I have said, that the circumstances I just mentioned increase the energy of the pile momentarily: its activity afterwards diminishes, but less by its exhaustion than by the alteration

alteration of
the plates.

Decomposition
of the salt.

Why light
does not heat
the air.

The current
transfers non-
conductors
with most ra-
pidity.

Substances
that have the
strongest affi-
nity prevented
from exerting
it by the force
of the current,

alteration the plates experience. I have noted down a multitude of facts respecting the circumstances that increase or diminish the activity of the pile, which would be sufficient perhaps to form the basis of a theory in this respect.

To return to my subject, from which I have wandered far. Not only does the acid transferred in Pacchiani's experiments correspond with that of the salt in the solution with which the pasteboards are moistened; but the base, that fixes the acid, is the same as that of the salt. If this base be a fixed alkali, or an earth, it passes without being decomposed: but if it be a metal, this is partly reduced by the same cause as decomposes the acids with known radicals, and is more difficult to be transferred. It is a singular effect of the galvanic current, to transfer substances so combined with it, or rather with such great rapidity, that it traverses with them substances for which they have the greatest affinity, without leaving them power, or rather time to enter into combination with them. It is nearly as Ducarla supposed in his excellent paper on *perfect fire*, that light traverses the air, or any other diaphanous medium, without heating it, because it does not remain long enough in a place to exert its calorific powers. The figure is sufficiently just, but the cause assigned is false: this action of traversing diminishes, but does not cease, when the current is transmitted through an interposed substance, that is but a semiconductor. What is farther singular is, that the same current transfers with more rapidity and facility substances eminently insulating, as the sulphur of alkaline sulphurets, the resins of ethereous and alcoholic tinctures, &c*. I first observed and made known, not only that the alkali or earth of a muriate employed in the moistening solution will traverse muriatic acid, or any other acid interposed to the current; or that these acids will equally traverse an earthy or alkaline solution, interposed in the same manner, without entering into combination with it; but that the two principles of the salt employed meet in the course of the circulation without uniting,

* If the current act mechanically, and not chemically, it is not singular, but natural, that it should exert a greater impelling power on the particles that resist its course, than on those over which it glides easily. C.

Before

before they return to the point of departure on the decomposing plate; and this after having travelled together, though no doubt with different velocities, along the negative wire. For this effect the wires must not be interrupted by substances, that the current unites, or separates; for in this case it quits, at least in part, the substance it transports, either to enter into combination with one of the principles of the substance it would decompose, or to transform itself into heat, and raise the temperature of the principles it would dispose to unite.

if there be no connecting substance, that the current unites or separates.

I have said, that the two principles of the substance employed in the moistening solution meet without uniting. Not that I mean to assert they proceed in opposite directions; for nothing would be more absurd than to suppose, that the negative of the pile, which is a nonentity, a negation of quality, a privation of power, can convey a substance; or even that a body can slide along a wire, because it is deprived of the electric fluid: but the two principles are carried along by the fluid, which passes along the positive wire to return to the plate from which it set out, but carried with unequal velocities. This difference of velocity would not be sufficient to prevent these principles from meeting with each other, unless the separation was made in a single instant, or all at once: but as this separation takes place in an infinite number of instants, and almost without interval, it is not possible that the acid for example, which is conveyed first, or with the greatest velocity, since it arrives first at the interposed tube, should not gain upon the alkali separated immediately before it, and pass by it, or unite with it, if it could unite with it. The current then passes along the two wires, and in the same direction, as if they were a single wire; and the negative state advances on the wire termed negative only in proportion as the positive state withdraws itself from the pile to advance on the positive wire. These wires then are to be considered only as prolongations of the two opposite states of the apparatus.

The elements separated do not properly meet, but one passes by the other.

The acid passes by the alkali.

To return to the experiments of Pacchiani. Do you not perceive, that they, who reject his conclusions, yet admit the production of an acid, are chargeable with a forced explanation, when they ascribe the origin of this acid to the substance

The acid not derived from the interposed pieces,

or the alkali
from the glass.

stance of the interposed pieces, and that of the alkali to the decomposition of the glass; though the experiment, however long continued, does not cease to furnish the same acid and alkali, provided the activity of the pile be not diminished by the oxidation of one of the plates; which, as I have already said, appears to be the determining agent of the decomposition of the impregnating salt; and though the glass, into which the acid is received, loses nothing of its polish?

Both are produced without these,

but not without a saline solution.

Besides, the production of an acid and alkali equally takes place, if we use wires or slips of metal for conductors, and a metal vessel for a receiver: and it does not take place at all, if we operate with pure water instead of muriates, whether we use animal or metallic substances to conduct the current, and receive the acid in glass or in metal.

Girtanner's hypothesis, that hydrogen is evolved from muriatic acid on dissolving a metal.

Since the pretended decomposition of water into muriatic acid by the pile, the opinion of Girtanner has been revived, which I controverted in the 1st volume of the *Memoirs of the Institute*, and according to which the hydrogen, evolved in solutions of metals by the muriatic acid, is said to come from the acid, and not from the water. But if, according to the assertion of Pacchiani, "water be superoxygenated muriatic acid, or oximuriatic acid with the addition of a fresh quantity of oxygen," still it would be this fluid, and not the acid, that must yield the oxygen; for, according to every law of affinity, it is the last portion of a principle combined, or its supercombined part, that separates first, being retained by a less powerful attraction.

Pacchiani's, that water is superoxygenated muriatic acid.

The pile should not be insulated.

In galvanic experiments great care is always taken, to insulate the pile, as if this apparatus, composed of a negative and a positive part, or a succession of surfaces alternately charged and discharged, were not, like every other electrified substance, the natural conservator of its own charge. Besides, the pile is not only incapable of losing any part of its fluid by communication with the ground, but its charge, which arises spontaneously, or without foreign accumulation, does not destroy itself on forming a communication between its surfaces, or opposite poles. In the charge of a body not insulated by itself, as a conductor, it must be insulated from the Earth, from which the fluid is extracted, and to which it has a tendency to return. In that of a bottle, plate of glass,

or

or other substance, the insulation is made by the substance itself; and, to preserve it, the charged part must always be kept from any communication with the ground. But the pile has nothing to do with the ground: it can neither take any thing from it, nor give any thing to it, its charge being fixed by its discharge, and *vice versa*. It is only in cases where the pile would be exhausted by furnishing its fluid to decompose a substance, as to convert the two elements of water into gas, &c.; or to compose one, by raising the temperature; or lastly to make an explosion through air, and for this purpose transform itself into light and heat; that the pile can reestablish its charge, or rather its natural state, at the expense of the ground. In this case its not being insulated would be advantageous, rather than otherwise: and in fact, as I have already made public, an uninsulated pile retains its energy much longer than another that is insulated. I shall not say however in what way I conceive this restoration can take place in an apparatus, which for its revival has the resource of decompositions of heat [*décompositions caloriques*] by its plates.

Uninsulated
pile retains its
energy longest.

I cannot conceive how in France, the seat of an academy that admits only strict deductions, the dualist electric theory, or that of two fluids, is still maintained in preference; and this in a more extravagant sense than that of Symmer himself, who was the author of it. In the hypothesis of two opposite electricities, what in fact are two fluids of the same nature, that repel each other? What are opposite powers (which naturally must endeavour to destroy each other, without being much concerned about the substances, that are interposed between them), which, applied to the particles of bodies, tend with the greatest energy to disunite their elements? as if the electric fluid, which in the case of its being set in motion, or in its operation, does not adhere to bodies, unless it enters into combination with them, but glides over their surfaces with the rapidity of lightning; and which in its state of rest keeps itself on the surface of the bodies to which it adheres; and diffuses itself in zones, or strata of opposite states, which mutually enchain or destroy the activity of each other, in bodies to which it does not adhere; could exercise the least action in these cases. This supposed conflict

Hypothesis of
two electric
fluids.

conflict is a little like that formerly admitted between alkalis and acids, on which their effervescence was imagined to depend. For how can we suppose an action without the direct intervention of the acting substance, and without admitting in this substance an affinity, if not of combination, at least disposing, or of indirect or remote combination with one of the principles of the substance on which it acts; simply to ascribe its decomposition, and that in its most intimate connexions, to a conflict between two opposite powers? a conflict that manifests itself in no way, that all the phenomena contradict, that resembles no other action known, that want of reflection alone could suggest, and that is founded only on a big word without meaning, on a fine phrase, under cover of which so many falsities have passed without examination from age to age. It is not thus in nature, where substances and powers of the same quality attract each other, are confounded together, and concur to the same end; and where substances and powers of opposite qualities either attract each other to combine peaceably, or are indifferent to each other. If the action ascribed to the electric fluid could exist, if a passive action without object and without end were not a contradiction in terms, it would be to reduce the influence of this powerful fluid to something very trifling, it would be to make it perform a very inferior part.

Substances or powers of the same properties coalesce; those of opposite qualities attract or are indifferent to each other.

The author's theory.

The following is nearly the true mode of action of the electric fluid. I repeat what I have said elsewhere in giving it, but when we perceive the wisest heads going astray, the principles, that must cure them of their error, cannot be too frequently brought forward.

Electricity combines with an element and forms a gas,

When the electric fluid, either of the common machine or of the pile, decomposes substances, one or more principles of which are bases of permanent gasses, it enters into combination with these bases, which it convertes into gas in the same manner as light does. Thus it decomposes water, nitric acid, ammonia, metallic oxides, &c. It acts on this occasion as a true chemical power, since it destroys combinations that exist by chemical affinity. In these decompositions it enjoys an advantage over light, being in the state in which permanent gasses appear to contain caloric, and in-

more directly than light:

to

to which light must transform itself to be able to unite with their bases. The nature of this power is farther confirmed by that which is requisite to disengage permanent aeriform combinations.

In the compositions and decompositions of substances by one another, it acts sometimes by concurrent at others by disposing affinity. In the first case it unites with one of the principles of the substance, which principle must always be a base of a permanent gas; and in the second it merely favours the action of a second principle by acting as heat. Thus it occasions the oxidation of metals by water, by converting the hidrogen into gas, &c.; and calls into action the affinities of all substances in the same manner as caloric, by diminishing their attraction of cohesion, or dissolving them.

It is as caloric too, that it effects direct combinations, such as those of the bases of water, &c.

It decomposes, and that particularly in the current of the pile, certain insulated substances, on the principles of which it exerts no attraction but that of conveying with different velocities. This action, I confess, is singular, and supposes in the fluid a great attraction of adhesion to the principles of these substances, as it can make them follow the rapidity of its translatory movement. The decompositions of salts with indecomposable acids and bases particularly take place in this way. I have remarked, that this action is scarcely at all exerted by the pile, when the communication is established by means of thick wires, or other substances with an extensive surface.

To effect combinations that require a red heat, as most combinations, &c., the fluid should be made to pass through a stratum of air, in order that it may concentrate itself sufficiently to force this passage through a medium, which refuses it as a nonconductor, and thus establish itself in the state of light and heat. Thus it is in quality of these two modifications of caloric, that it produces inflammations, &c. The presence of light appears to be necessary in these operations, to impart the luminous constitution to the electric form caloric, which converts the oxygen into gas.

It appears, that, every time the electric fluid no longer finds occasion to exercise its attraction of adhesion and expansion and in part assumes the state of light, unless

assists in the composition or decomposition of substances by a concurrent, or by a disposing affinity:

as heat effects direct combinations:

decomposes substances by carrying along their elements with different velocities:

effects some combinations by concentrating itself into the state of light and heat:

when its tendency to expand is so restrained by a nonconducting medium, as to exercise the attraction of adhesion.

Objections to the hypothesis of two fluids,

which is shown to be fundamentally wrong.

Franklin's theory.

Always acts by attraction,

even where this appears to alternate with repulsion.

pansion on bodies, as in gliding along a nonconductor, in traversing a nonconducting medium, in a vacuum, &c., it assumes in part the state of light. This attraction of adhesion is always the result of the tendency of the fluid to expand, and of the restraining action of the air.

To return to the theory of electricity, beside that Symmer's, namely the hypothesis of two fluids, or rather of three, the resinous, vitreous, and both combined, is more complicated; and that we perceive no difference between the two fluids in respect to their action, for the operator can only judge by the comparison and opposition of these fluids, whether he have excited the one or the other, a circumstance which has nothing analogous to it in natural philosophy; this hypothesis is fundamentally overturned by the fact, that glass and resin excite both the electricities indifferently, accordingly as they are rubbed with substances more or less conducting than themselves. The Franklinian theory on the contrary is simple, adapted to the nature of things, and agrees with the received doctrine of the exercise of material powers; its attractions and repulsions are effects of the same cause, and the natural result of an elastic fluid, eager for an equilibrium, the opposite states of which, in order to find this equilibrium, rush from the place where it is plus toward that where it is minus, carrying with it the substances on which they are excited, if these substances be light, and freely suspended, or movable. It is never repulsion therefore, but always attraction, that causes the motion of these substances, which in the ball electrometer separate by the communication of positive electricity, for the purpose of depositing the excess of their fluid on the sides of the glass, in which this excess has excited a state of defect; and by the communication of negative electricity, to repair their defect of fluid from the same sides of the glass, in the substance of which this defect has excited a state of excess. The same thing takes place in the air: besides, the separation in both cases is necessary for the formation of the opposite atmosphere, without which no electrical charge can be established or retained, and no discharge take place. In the other cases of alternate attraction and repulsion, as in that of an insulated ball immersed in the atmosphere of an electrified

electrified substance, this ball only acts the part of a messenger, or serves as an instrument to reestablish an equilibrium between the opposite states. Hence it was erroneous to oppose to the theory of one sole fluid, with the affectation of so much confidence, the phenomena of repulsion between two substances negatively electrified, and the direction of a needle to the same point, whether moved by a current flowing to it, or a current escaping from it. In one case it moves to get rid of the fluid received, in the other to regain that which has been taken away.

No intelligent partisan of the theory of a single fluid ever said, that the air condenses round a substance negatively electrified, or electrified by subtraction.

Air never condensed round a substance electrified minus. Girault's experimentum crucis.

In the experiment proposed by Girault to decide between the two theories, an experiment that has been a thousand times made, the fluid is not transferred from one coating to the other, but diffuses itself in the void space, which no longer offers the resistance, on which its condensation and its adhesion to substances depend: yet as in this experiment the vacuum cannot form at once, the electric fluid separates in succession as the air rarefies, and prevents us from observing whether it flow from the exterior or interior surface of the bottle. For this experiment it is not necessary, to charge the bottle at a machine with a plate of resin: for to charge it negatively within, it is sufficient to present its outer coating to the conductor, while its interior coating communicates with the ground, or to charge it in the usual way, presenting its hook to the cushions of a machine, the conductor of which is not insulated. In electrical experiments made in vacuo, the fluid that ceases to be applied is transformed into light, traverses the receiver, and is dissipated in the air.

I have written you insensibly a long letter, yet what I have said has not been the more interesting on this account. My head and my papers are filled with memorandums of facts and ideas, which my occupations do not allow me to set in order, and which render me diffuse when I have occasion to speak of them.

V.

Description of the Process I have employed to ascertain the existence of Alumine in Meteoric Stones, by B. G. SAGE, Member of the French Institute, Founder and Director of the First School of Mines.*

Fusion of a stone with alkali alters the nature of some of its principles.

Alumine in meteoric stones.

That of Salles contained malleable iron.

Treated with sulphuric acid.

MARGRAFF and Bayen proceeded to the analysis of stones by vitriolization, because they had found, that fusion through the medium of alkalis altered the nature of some earth. This appeared to me unquestionable, since chemists the most justly celebrated, as Klaproth, Fourcroy, Vauquelin, &c., who have given analyses of the meteoric stones, named aerolites by Mercati, have not mentioned the alumine, which I can affirm exists in them: for, having vitriolized some of the meteoric stones of Aigle and Salles, near Villefranche in the Lyonese, I obtained alum from both, but in unequal proportions, since the aerolite of Aigle yielded me near a fourth, while that of Salles did not afford above an eighth.

I powdered and sifted through a silk searce some of the meteoric stone of Salles, an eighth part of which was irreducible to powder, because it contained portions of malleable iron, attractable by the magnet. This, being fused with glass of borax, produced ductile iron, that had the brilliancy of the purest steel when passed through the flattening mill; while the malleable iron obtained from the aerolite does not assume an equally brilliancy after being laminated. A portion of this iron had coloured the glass of borax black, and rendered it attractable by the magnet.

To vitriolize the magnesia and alumine, which make part of meteoric stones, I introduced into a retort eighteen nominal cwt. of the aerolite of Salles, powdered and sifted; poured in an equal quantity of concentrated vitriolic acid; and proceeded to distil to dryness in a reverberatory furnace. Sulphurous acid was at first evolved, accompanied with yellow sulphur, which was found in the proportion of a

* Journal de Physique, vol. lxxvi, p. 460.

thirtieth in the meteoric stone. At the bottom of the retort was left a grayish mass, which, after having been diluted in three parts of water, produced a sensible heat.

This solution, when filtered, was of a fine green colour, owing to the nickel and iron. When evaporated, it produced by refrigeration tetraedral prismatic crystals of a light green.

Solution filtered and evaporated.

The crystallization being confused, I dissolved the salt, and obtained crystals of two distinct forms. Those of vitriol of magnesia were in tetraedral prisms, intermingled with crystals of alum, exhibiting octaedra bisected diagonally. Both these salts had a green tinge.

Sulphates of magnesia and alumine crystallized.

The small quantity of alum produced by this first vitriolization showed me, that only a part of the alumine was acted on. I therefore distilled the dried residuum, which was diminished five nominal cwt. with eighteen nominal cwt. of vitriolic acid. The residuum, after being lixiviated, afforded me a solution less tinged with green; which, being evaporated, yielded me more alum than the former. The residuum of this lixiviation being dried and weighed, I found there were two nominal cwt. of alumine and magnesia vitriolized in this operation.

Residuum treated afresh with sulphuric acid.

In order to disengage the last portions of alumine and magnesia, that remained still interposed among the silix, or pulverized quartz, I distilled the residuum a third time with twelve nominal cwt. of concentrated vitriolic acid.

Treated a third time with sulphuric acid.

After what remained in the retort had been lixiviated, filtered, and dried, I found that the sulphuric acid had vitriolized two more nominal cwt. of alumine and magnesia. By distilling the residuum a fourth time with vitriolic acid, I satisfied myself, that it contained no more alumine or magnesia. Nothing remained in the retort but very white silix, weighing nine nominal cwt.

More alumine and magnesia.

When I analysed the meteoric stone of Aigle, I added together the solutions of the three vitriolizations, which, on being evaporated, produced me at first alum, and afterward vitriol of magnesia. But in the analysis of the aerolite of Salles, I evaporated the solutions of the three vitriolizations separately: and hence I learned, that the sulphuric acid vitriolized the magnesia first: since the first lixivium yielded

Meteoric stone of Aigle.

but

but very little alum, while the second and third produced more. This alum comports itself in the fire like that of the shops. It swells up, and assumes a reddish tinge, owing to the martial vitriol it contains.

Proportions of the component parts vary except the silix.

These comparative experiments show, that the proportions of magnesia and alumine in meteoric stones are not always the same. Those of iron varying too, it is not possible to determine precisely the quantity of these different substances, that make part of the aerolites: but the silix pretty uniformly afforded me half the weight of the meteoric stone, of which sulphur constitutes only a thirtieth.

Treatment with sulphuric acid necessary to a perfect analysis.

The existence of alumine in meteoric stones being confirmed by means of vitriolization, which likewise detects the presence of this earth in hornblende; though it escapes us, when we proceed to the analysis of these substances by means of caustic alkali, since the able chemists I have cited make no mention of it; it is necessary, for the purpose of an accurate analysis, to have recourse to the two methods.

Silix.

The silix, or quartz in a state of division, mentioned as an integrant part of most stones, may be nothing, in many cases, but the result of the decomposition of igneous salts with base of natron, the fixed alkali of tartar having more affinity with acids than natron has.

Meteoric stone cut and polished.

The fracture of meteoric stones making known but very imperfectly the arrangement and brilliancy of the native iron they include, I resolved, in order to examine it on a large surface, to get a vase turned from an aerolite of Salles near Villefranche, in the Lyonese. It was found difficult to fashion, because irregular splinters broke off before the tool; in consequence recourse was had to the file, and to rubbing it dry on a plate of cast iron covered with powdered gritstone and emery.

The last polish was given with emery and Venetian tripoli, using no water, that the iron might not rust.

Its appearance in this state.

The vase, which I offer to the inspection of the Institute, exhibits parcels of iron of irregular configurations, which have a silvery lustre, intermingled with very small spots of a greenish yellow, disseminated in a quartzose gangue of an ashen gray.

VI.

Letter from Mr. RAMPASSE, formerly Officer in the Corsican Light Infantry, to Mr. CUVIER, on a Calcareous Breccia containing fossile Bones found in Corsica.*

SIR,

I Mentioned to you a calcareous earth containing bones which I had found in Corsica, and which might not appear indifferent to a man of science ; but I did not enter into any particulars on the subject. Having at present before me the memorandums I made of my geological travels in that island, I shall give you an account of that very curious earth, acquainting you with all the circumstances that gave rise to its discovery.

Visiting the north part of the environs of Bastia, that faces the east, and desirous of visiting likewise the upper part of the chain separating the gulf of San Fiorenzo from that of Bastia, I took my departure from the seashore near the Jesuits tower, distant from the city about a mile and half. I ascended a small narrow hill, the steeply sloping sides of which are full of rocks, some in their natural situation, others loose. When I had proceeded on the hill to the distance of about a mile and half from the sea, and about two hundred yards above its level, being on the side opposite to that on which I began my walk, a considerable ledge of calcareous stone presented itself to my view in an oblique situation from south to west, steep, and having on it the appearance of an irregular column, reaching from top to bottom, with a brownish red ground ; and at a distance three others much shorter, being only two or three feet high. The rest of the rock was of a blue ground mixed with white. On examining this vast mass of stone, I perceived that a quarry had formerly been opened in it ; and desirous of knowing at what period, I inquired concerning it of some of the vine-dressers, among whom were some old inhabitants of the villages of Santa Lucia and Leville, near the spot. They told

Calcareous earth containing bones in Corsica.

Hill near Bastia.

containing a stratum of calcareous stone

intersected perpendicularly by a stone of a different appearance.

Formerly quarried.

* Journal de Physique, vol. LXV, p. 426.

me, that in 1774 a great quantity of stone had been taken from this place, to build several houses, and walls of enclosures among the surrounding vineyards. In fact this calcareous mass had been wrought so much in one part, that it was not more than two or three feet thick; while the other part, which was yet untouched, was twenty-five or thirty: which led me to judge, that the general height of the whole mass might have been about five and twenty feet.

The stratum
described.

This ledge, about seventy or eighty yards long, was intersected in some parts from top to bottom by a reddish brown earth; very hard, and as it were enchased in the rock, as I have said, in the shape of irregular columns. Before the opening of the quarry, this earth exhibited four columns, one of which alone remained entire, and sloped from its middle to the top. The other three exhibited only about two feet of the shaft, reckoning from the base, the rest having been cut away with the rock. Each of these columns was from three to four feet broad, and from fifteen to eighteen thick. They, as well as the rock which appeared to enclose them, were imbedded in the mass of earth at their back, throughout the whole of the extent of the ledge both in length and height; which must formerly have exhibited the appearance of a very extraordinary intercolumniation, both on account of the colour of the earth, which was very different from that of the stone, and from the irregularity of these columns, which had altogether the appearance of so many distorted walls, constructed in the interior of the stony mass.

Similar ap-
pearance in
other strata.

I had before had an opportunity of observing a similar natural architecture in other calcareous ledges still more extensive than this, as those situate to the south near the city of Bastia, on the estate of Messrs. Pallavicini of that city, in which appears, independent of a resemblance of pillars of a blackish gray, an earth not so hard as that mentioned above, of a different colour too, and not so thick, which is arranged horizontally in strata between the beds of stone, but containing only small nodules of the same earth harder than the body of the earthy mass.

From the mines sprung in the quarry, this brownish red earth, being blown up with the rock to which it appeared

to adhere, was scattered about the bottom of the quarry in large blocks. These blocks, when blown up, had left large vacancies in their old place, in which were observable a number of cavities five or six inches in diameter.

This vast ledge is in the midst of a wood of wild and domestic olive-trees, on the ridge of the hill I have mentioned, where it forms a sort of little mountain. It is surrounded also by a number of blocks of stone, likewise calcareous; some of which, having their angles broken off, appear to have already undergone a change of place; while others perhaps have come from the ledge itself, for there is no doubt, that it was formerly more extensive, than when the quarry was opened in it, since every thing indicates a derangement of things in this place. This ledge, of a circular form; rests principally on a bed of the same reddish brown earth, perfectly resembling that which composes the columns, and a blackish vegetable mould forms its base. The north and east are the two points, toward which the part wrought looks; and that untouched faces the west; so that the whole ledge forms a semicircle.

On attentively observing this calcareous mass, I perceived that a number of little bodies, which appeared to me homogeneous, were embedded as it were in the brownish red earth, and, being equal in hardness to the stone, induced me to give it the name of calcareous breccia. I noticed three different kinds of these small bodies: some of a calcareous nature, and a rhomboidal figure, inserted in it in groupes; others of a refractory nature, with the aspect of a foliaceous granite, containing little laminæ of mica in a state of alteration; and lastly little round long bones, perforated at one end, and destitute of spongy texture; which appeared to me tibia's of some large bird or small quadruped. Continuing my remarks; and desirous of being more fully acquainted with the contents of this earth, I attempted to break several blocks, to get a good specimen. Not being able to accomplish this without a great deal of trouble and exertion, and my curiosity not being satisfied, I bethought myself of recurring to the cavities and vacancies, which the blowing up of the rocks had laid open. In fact by this means I was more successful, and with less labour; for

Situation of
the stratum;

The interposed
stone contain-
ed

fragments of
other stones;

and bones.

without much trouble I separated with my hammers from these cavities, the sides of which were already shaken by the explosion of the mine, the fine specimens I brought away, and which I take great pleasure in sending to you for your examination.

Two specimens.

In the large specimen, and in the small one which was broken off afterward, may be distinguished a head; a pretty large rib, the spongy texture of which is converted into earth; and other bones, that appear to have belonged to small quadrupeds. The leg, thigh, and foot bones, and other bony parts, observed in other places, appear to be those of birds; and lastly in other specimens are portions of shells, that I believe to be of the helix genus.

A similar stone found at Gibraltar, Cette, and Nice.

This earth, or calcareous breccia, having led me to various reflections, I would willingly add to the circumstances I have related important details, to which its discovery leads; but it would swell my letter too much, to trace the causes that have produced these interesting facts. I shall only say, that a similar earth has been found at four different points of Europe, Gibraltar, Cette, Nice, and Corsica: and as these four points, compared with all Europe, may be considered as one, I conceive, that the discovery of this earth in Corsica not only indicates this island as the point, to which the eye of him who would observe the grand revolutions, that every thing announces to have existed, should be turned; but also becomes a fertile source of luminous ideas respecting those great catastrophes, that have taken place at a very remote period in this part of the Mediterranean. Time, and tours undertaken and pursued without interruption, can alone acquaint us with the extraordinary events, some proofs of which have already been found by enlightened men.

Mr. CUVIER's Answer to Mr. RAMPASSE.

The bones belong to the genus lagomys.

I have been greatly interested, sir, by the observations you have communicated to me respecting the bony breccia of Corsica, and have examined with the greatest care the bones they contain. Among them is a head well characterized,

terized, which must have belonged to the genus *lagomys*, of which there are at present but three species known, all of them discovered in Siberia by Pallas.

It would be a subject of some curiosity, to examine these breccia still farther on the spot, and obtain from them a larger quantity of bones, in order to discover whether these animals were buried there in great numbers; whether the bones of other animals accompany them, and, if so, of what countries these are natives; and lastly if their bones are worn, broken, and have the appearance of having been brought from a distance.

Farther inquiry, respecting them pointed out.

You are aware, sir, without my entering into the subject, how much light the solution of all these questions would throw on the history of the revolutions of our globe, at present so obscure.

VII.

Extract of a Letter from Professor PICOT of Geneva to the Editors of the Bibliothèque Britannique.*

I Shall first speak of that beautiful comet, which excited such a lively and general curiosity last year [1807]. Discovered in September, immediately after passing its perihelion in the constellation of the Serpent, it travelled the following months nearly at the rate of a degree a day in those of Hercules and the Lyre. Imperceptibly diminishing in lustre as it increased its distance from us, and even ceasing to be visible to the naked eye, it was followed only by a few astronomers in those intervals, when the fogs and winds permitted observations to be made. Mr. Olbers availed himself of these favourable moments, and observed it till the 19th of February, when his labours were interrupted by an illness, from which he was not recovered the 28th of

Comet of 1807.

* Journal de Physique, vol. LXVII, p. 193.

April, when his letter to me is dated. Mr. Bessel, the coadjutor of Mr. Schroeter, in his fine observatory at Lilienthal near Bremen, was able to follow it till the 24th of February; and by him were calculated the elements, that will appear in this paper, from the observations made at Bremen and Lilienthal.

Its period per-
haps 1900
years.

He imagines, that he can determine the period of the return of this comet to its next perihelion. According to him its revolution in its orbit is 1900 years: but Mr. Olbers says, that we cannot depend on the accuracy of this determination. It is much to be wished, that he, or some other astronomer, would collect at leisure all the accurate observations of this beautiful comet, made at different places, revise these calculations, and endeavour to arrive at a probable result. The reappearances announced of two comets; that of 1456, which has been seen four times, and that of 1532, which has been seen twice; demonstrate the general proposition, that these returns may be predicted. If however Mr. Bessel be near the truth with respect to the length of the revolution of this comet, the approaches it may make during so many centuries to the large gravitating bodies belonging to our system may occasion perturbations in its course, of which no calculation can be formed.

Elements of
the last 21.

To finish this article of comets, I shall annex the elements of the last twenty-one from Mr. Olbers. They are valuable, both because he himself has observed them, and calculated several of their elements, namely, those marked with a star; and because they determine with more precision than the *Connoissance des Temps* one of the most essential circumstances for the calculation of their periodical revolutions, namely, the precise instant of their passing their perihelion. I set out with the numeration of Pingré in his *Cométographie*, to indicate the number answering to each of these last comets in the complete catalogue of those, the orbits of which are calculated.

Elements of the last Twenty-one Comets, according to OLBERS.

No.	Year.	Passage through the Perihelion in mean time at Paris.	Longitude of the Perihelion of the Comet.	Perihelion Distance of the Mean of the Earth being 1.	Longitude of the ascending Node.	Inclination of the Orbit.	Direction of the Motion.
		h. ' "	s. ° ' "		s. ° ' "	° ' "	
77	1790	Jan. 15.	5 15 0	0-75310	5 26 11 46	31 54 15	Retrogr.
78	1790	Jan. 28.	7 45 30	1-06329	8 27 8 37	56 58 13	Direct.
79	1790	May 21.	5 56 15	0-79796	1 3 11 2	63 52 27	R.
80	1792	Jan. 13.	13 44 13	1-29302	6 10 46 15	39 46 55	R.
81	1792	Dec. 27.	7 56 27	0-96683	9 13 14 44	49 7 13	R.
82	1793	Nov. 4.	20 21 0	0-4634	3 18 29 0	60 21 0	R.
83	1793	Nov. 18.	15 38 0	1-5045	0 2 20 0	51 56 0	D.
84	1795	Dec. 15.	8 29 50	0-24379	11 23 14 0	22 10 0	D.*
85	1796	April 2.	19 55 6	1-57816	0 17 2 16	64 54 33	R.*
86	1797	July 9.	2 40 31	0-52661	10 29 15 37	50 40 34	R.*
87	1798	April 4.	12 7 37	0-48459	4 2 12 21	43 44 42	D.*
88	1798	Dec. 31.	22 5 15	0-77479	8 9 30 2	42 14 52	R.*
89	1799	Sept. 7.	5 43 26	0-84018	3 9 27 19	50 57 30	R.*
90	1799	Dec. 25.	19 3 50	0-26688	10 26 27 18	77 0 47	R.
91	1801	Aug. 8.	13 0 0	0-249	1 12 8 0	20 20 0	R.*
92	1802	Sept. 9.	21 32 29	1-69411	10 10 15 39	57 0 47	D.*
93	1804	Feb. 13.	14 16 16	1-07117	5 26 47 58	56 28 40	D.
94	1805	Nov. 18.	3 14 27	0-37862	11 14 37 19	15 36 32	D.
95	1805	Dec. 31.	6 21 1	0-89193	8 10 33 35	16 30 32	D.
96	1806	Dec. 28.	22 2 10	1-08193	10 22 18 37	35 4 5	R.
97	1807	Sept. 18.	7 59 48	0-64648	8 26 46 3	63 10 53	D.

General conclusions.

In this table the reader may perceive,

1. That, during the last eighteen years, observations on comets have been more frequent than ever; and that the vigilance of astronomers to discover new ones has equalled that they have employed in discovering also new planets.

Perihelion distance.

2. That, of the comets observed, those which in their perihelion have approached the sun nearer than the Earth's mean distance from it are double the number of those, the perihelion of which exceeds this distance. Four of them have approached nearer to the sun than the tenth of the Earth's mean distance, and four others nearly within one fifth of it.

Direction.

3. That, with regard to the direction of their motion, twelve have been retrograde, and nine direct.

Longitude of the node and perihelion.

4. That, as to the longitude of their ascending node, and of their perihelion, it answers indifferently to the 360 degrees of the circle, on which that longitude is reckoned.

Difference between comets and planets.

Nothing in the solar system is more remarkable, than the indeterminateness of the places of the orbits of comets, of their inclination in all angles to the plane of the ecliptic, of their eccentricities, of the place of their perihelion, and of the direction of their movement, if compared with the precise determinations to which the planets are subjected. The orbits of the latter are nearly circular, and very little inclined to the plane of the ecliptic: all the planets, both primary and secondary, move in the same direction, from west to east; and those, the rotation of which we have been able to observe, turn on their axis in the same direction. Thus the planetary system, says Mr. Laplace, in his *Système du Monde*, displays to us forty-two movements in this direction, and it is four millions of millions to one, that this arrangement was not the effect of chance. Different final causes therefore must have presided over the different formation and destination of the planets and comets.

Not owing to chance.

VIII.

On the Influence the Shape of a Still has on the Quality of the Product of Distillation: by Mr. CURAUDAU, Member of the Pharmaceutical and several other Societies.*

WHEN Mr. Chaptal pointed out the fault of our common stills, and proposed to substitute for them broad and shallow alembics, I was one of the first, to consider the reform as very useful, and at the same time highly conducive to the interest of the distiller. Accordingly, having had occasion to write on the same subject, I proved, that I coincided in opinion with Mr. Chaptal, by extolling the advantages, that shallow stills possessed over deep ones.

Though I had no foundation for my opinion but theory, and the particulars advanced by Mr. Chaptal in support of the system he proposed, I was far from thinking, that I should have to retract the assertions I had made, and that experience would destroy the plan of reform, the adoption of which I had sought to promote.

However, as it is the duty of a man, who studies useful improvements in the arts, not to compromise the progress of science, or sacrifice to selflove whatever tends to correct the errors, into which he may have fallen, I hasten to communicate to the physical and mathematical class of the Institute the observations, that have arisen from the objections made to me by those, who have employed shallow stills.

In deep stills, the liquor, at a certain time, receives more heat, than it gives off by evaporation: the temperature then may rise, till it reaches the term at which the ebullition is complete, an essential condition for effecting the combination of the alcohol with the aroma of the wine, before it is separated from it.

No doubt shallow stills greatly shorten the time of distillation; this is a fact, on which all distillers agree: but they

Shallow stills
proposed by
Chaptal,

and recom-
mended by the
author:

but his opinion
now changed.

In deep stills
the liquor
heated more,
and finer fla-
voured.

Distillation
quicker in
shallow ones.

* Sonnini's *Bibliothèque physico-économique*, 1808, tom. I, p. 106.

say too, and this cannot be disputed, that the brandy obtained in this method contains nothing or next to nothing of that aroma, which is so grateful to the smell, and communicates the agreeable flavour, that distinguishes well made brandy.

Experiments
proving

It is this difference in the quality of the products, that has engaged the attention of distillers. I thought at first, that they might have been deceived by their prejudices, and boldly disputed their opinion: but finding, that shallow alembics fell more and more into disrepute, I resolved to examine for myself, whether the objections made to them were well founded. What I thought it particularly necessary to ascertain was, whether the difference in flavour between brandies distilled in alembics of the different forms were sufficiently perceptible, to authorize the preference given to one over the other. Accordingly I subjected to distillation a quantity of wine, part in a shallow alembic, part in one of the common construction.

the inferiority
of the shallow
still.

When I had finished the distillation, I examined both sorts of brandy, and gave them to different persons to taste, all of whom, as well as myself, uniformly gave the preference to that produced from the deep still. Thus I was convinced, that the objections of the distillers were not the result of unfounded prejudice; and that the difference observed in the products of two analogous operations must depend on the circumstances of the evaporation; which were not the same in the two stills, since I satisfied myself, that, in the common still, the evaporation of the spirit does not begin to be very copious, till the heat is 70° or 75° of Reaumur [190° or 200° F.], while on the contrary in the shallow still it is very abundant from 45° to 55° [133° to 156° F.].

Difference of
the evaporating
heat.

This the cause
of difference in
quality.

This difference in the intensity of the heat produced, at the moment when the alcohol separates from the liquor that contains it, appeared to me worthy of remark, and tending to explain why the products must differ. In fact, is it not well known in chemistry, that wine distilled at the heat of a vapour bath yields a spirit much inferior in quality to that, which is produced by distillation on a naked fire?

Experience

Experience proves then, that it is necessary, to bring the wine to boil, before the alcohol is abstracted from it. This boiling favours the reaction of the principles of the wine, and is the cause of a new combination by their mutually acting upon each other, which renders the spirit more aromatic and highly flavoured, than that obtained from wine to which a similar degree of heat has not been given.

To explain why the liquor cannot be raised to the same degree of heat in a shallow still, as in a deep one, it is sufficient to observe, that, in the former, the evaporation always keeps pace with the heat produced: in other words, if we increase the fire, we only accelerate the evaporation, without perceptibly increasing the temperature of the fluid.

Ebullition necessary.
Cause of the difference in heat.

Hence it is evident, that shallow stills are far from being well adapted to attain this end; and the circumstance, that is essential to fit them for a speedy evaporation, is here a defect, instead of an advantage, in proportion to its efficacy.

From what has been said we may conclude:

1. That shallow alembics, though very fit for the distillation of certain fermented liquors, may sometimes alter the quality of the products of distillation.

General conclusions.

2. That the inconveniences arising from the employment of shallow alembics in distilling wines arise from the facility, with which evaporation takes place in them.

3. That a high temperature is always necessary, to carry over the peculiar aroma of the wine, and perhaps too that arising from the action of heat on the principles of the wine.

4. That deep alembics ought to be preferred to shallow ones for the distillation of wine.

5. Lastly, that the best dimensions for an alembic, without regard to its figure, must be such, that the surface of the liquor heated shall be constantly greater than that from which the evaporation takes place. Thus for instance we may consider it as a rule, that the proportion between the two should be as four to one.

ANNOTATION.

ANNOTATION.

Shallow still
best for malt
or melasses
spirits.

Deep still for
aromatic herbs
or perfumes.

From the practical observations of Mr. Curaudau we may infer, as indeed he hints in his first general conclusion, that the shallow still is preferable, where the object is to prevent the peculiar flavour of the liquor distilled as much as possible from rising; as in distilling from malt, or melasses, the common materials in our country: and this not only on account of the saving in time and fuel, but of superiority in point of flavour. On the contrary, in respect to the simple or spirituous distilled waters, as they have commonly been called, where a full impregnation with the peculiar flavour of the vegetable substance employed is desirable, a deep still would appear to be preferable. The proper proportions for stills for some of the finer productions of this kind however may deserve a particular inquiry. C.

IX.

*On Vegetable Astringents. By JOHN BOSTOCK, M. D.
Communicated by the Author.*

Action of re-
agents on vari-
ous astringents.

WHILE I was engaged with the experiments on the combination of tan and jelly, the results of some of which I have already transmitted to you, I was led to observe the action of a number of reagents upon the different astringent substances which I employed. The conclusions that I have been induced to form are, in some respects, different from those adopted by the most approved systematic writers, as well as by those experimenters, who have particularly directed their attention to this class of bodies. I propose to confine my remarks in the first instance to the gall-nut; and having adopted this as a kind of standard, I shall afterward make a few comparative observations on catechu and the extract of rhatany, substances which have been considered analogous to galls in their chemical properties.

In

In Aikin's chemical dictionary* we have an account of the great diversity, which exists in the structure and appearance of different gall-nuts, a circumstance which appears previously to have been but little attended to. I have found my observations to correspond with those of the Mr. Aikins, and to be fully confirmed by my experiments. Although the same quantity of materials be employed, it is very seldom that two infusions of galls are obtained of the same strength, and I have found the difference amount to no less than $\frac{1}{3}$ of the whole weight of the solid contents. In general, however, if finely powdered galls be infused in ten times their weight of boiling water for two hours, a fluid is procured containing $\frac{1}{10}$ of its weight of solid matter. An infusion of the same strength will generally be obtained, if the powdered galls be macerated in ten times their weight of cold water for 24 hours. If powdered galls be boiled or infused in hot water, the fluid is commonly, though not always muddy, and does not become transparent, until it has been kept for some days, or even weeks, and a considerable part of its contents have separated from it, in the form of mould or sediment. The muddiness is not removed by filtering the fluid, and there is often considerable difficulty in passing it through the common bibulous paper. This muddiness renders the warm infusions improper to be employed in experiments that require any great degree of delicacy. If the galls be only coarsely powdered, warm water still produces an opaque infusion, but if successive portions of warm water be applied to the same galls, the infusions will gradually become less and less muddy, until after the 3d or 4th they will be transparent; but the period when the muddiness ceases is not the same in all cases. It is probable, that the muddiness in these instances does not depend upon any part of the galls which is originally insoluble, but upon some one of their constituents which is rendered so during the process; for if a transparent infusion of galls be slowly evaporated, and the residue be afterward digested in cold water, a perfect solution of the whole can no longer be obtained. Facts of this kind have been frequently noticed

Galls not homogeneous.

Infusions of them differ by one third of their solid contents.

Great portion soluble.

Decoction, or hot infusion, generally turbid, even after filtration.

Successive hot infusions less and less muddy.

Some part rendered insoluble by the heat.

* Article, Gall-nut.

Galls should be finely powdered.

Successive infusions necessary.

Proportion of insoluble matter.

Action of reagents on it.

in the analysis of vegetables, and have been generally ascribed to the extractive principle, which is said to become insoluble by the absorption of oxygen*. It seems, that in all cases, the more finely the gall nuts are powdered, the stronger is the infusion, which equal weights will produce.

A considerable proportion of the gall-nut is soluble in water, but for this purpose it is necessary, that several successive infusions be employed. Water will readily take up $\frac{1}{10}$ of its weight of the soluble part of the galls†; and yet if a quantity of the powder be infused with 20 times its weight of water, a 2d quantity will extract something which had escaped the first infusion. This circumstance is particularly noticed by Trommsdorf; for although he infused galls for three days in above twelve times their weight of water, yet it required four of these infusions to remove all the soluble matter‡. The proportion of matter, which remains insoluble after these successive infusions, has been very variously estimated. Mr. Deyeux speaks of the insoluble part as a very small quantity, without stating its amount§; while Mr. Davy informs us, that he had 315 parts left out of 500, or nearly $\frac{3}{5}$ of the whole||. In four different trials which I made with a good deal of care, I found the residues to be in the different cases nearly as $\frac{1}{7}$, $\frac{1}{8}$, $\frac{1}{10}$, and $\frac{1}{12}$ of the whole. In these experiments the number of successive infusions was from 12 to 14, and the quantity of water employed at each infusion was ten times the weight of the original quantity of galls. What is left is a dark coloured and hard body, upon which alcohol and the caustic alkalis have no action. Muriatic acid, by being boiled on it, breaks it down into small pieces, and is itself tinged of a light brown

* Fourcroy, Analyse de Quinquina, Ann de Chim. VIII, 122, & alibi.
——— Système, VII, 312.

Davy, Philos. Trans. 1803, p. 237.

Thomson's Chem. V, 107.

Aikins' Dictionary, Art. Extract, p. 422.

† Mr. Davy formed infusions, which contained between 1-7th and 1-8th of their weight of solid matter. Phil. Trans. 1803, p. 240.

‡ Thomson's Chemistry, II, 355.

§ Ann. de Chim. XVII, 11.

|| Phil. Trans. 1803, p. 251.

colour;

colour; potash throws down a very minute precipitate from the acid, while the residue is rendered quite black, and strongly resembles charcoal powder; a circumstance which seems to show, that the blackness of charcoal is not necessarily connected with the the process of combustion or oxidation.

Blackness of charcoal not connected with oxidation.

In forming infusions of galls it occasionally happens, that we obtain them of a bottle-green hue. Mr. Deyeux and Mr. Davy both mention this as occurring in the latter infusions, where the same galls have had repeated quantities of water poured on them*; but it never occurred to me to observe the green colour under these circumstances, while, on the contrary, I have met with it in an infusion of fresh galls, where no shade of green could be observed in any of the subsequent infusions. It is attributed by Mr. Deyeux to a green colouring matter, which he enumerates among the constituents of the galls; while Mr. Davy ascribes it to the gallate of lime. My observations lead me to question the accuracy of this latter opinion. In the first place it seems an almost decisive objection to it, that, if lime water be added in small quantities to the recent infusion of galls, so that the tan be not precipitated, the green colour is not produced; yet in this case the lime must be employed in saturating the uncombined gallic acid, and thus forming the gallate of lime. In one of the greenest infusions that I ever procured, the oxalate of ammonia did not produce the least effect, while the subsequent addition of the most minute portion of lime water immediately caused the precipitation of the oxalate of lime. If to the infusion of galls pure potash be added, the brown colour is at first rather increased, but after some time a shade of green becomes visible. The effect is much more speedy and more decisive where the carbonate of potash is employed; a similar effect is produced by lime water, except that the green is more of the gluceous hue. In all these cases the green colour is instantly removed by an acid; where potash has been employed the fluid acquires a reddish tinge, and where lime water was used, a delicate violet. The green colour always disappears

Green tinge of the infusion

not owing to the gallate of lime.

Effect of reagents in producing or removing the green tinge.

* Deyeux, ubi supra, p. 12; Davy, ubi supra.

by exposure to the atmosphere, and it is also removed by boiling, although in this latter case it is partly reproduced after an interval of two or three days, but finally disappears. There seems to be an analogy between these changes, and the effect of acids and alkalis on many other vegetable substances, they are rendered green by an alkali, and are reddened by an acid. But the resemblance does not hold good in every respect; for the alkaline mixture loses its green colour, although its alkalescent properties continue; and I have observed the green colour to be removed by lime and ammonia, while, on the contrary, I have obtained infusions, which have exhibited the green colour, and yet by the test of litmus have proved to be decidedly acid. The green colour, wherever it exists, is immediately destroyed by the acetate of lead*. With respect to the existence of lime in the infusion of galls, the experiments which I have made on the subject lead me to conclude, that, although it may exist in the gall-nut, yet it is not taken up by the water. I have added to the recent infusion of galls both uncombined oxalic acid, and the oxalate of ammonia, without any precipitate being produced. If the ammonia be in excess a considerable effect takes place, but this is to be ascribed to the union of the uncombined alkali with the tan.

No lime found
in infusion of
galls.

If the infusion of galls be kept for any length of time, it always becomes covered with mould, and a sediment also falls to the bottom of the vessel. The moulding has been attributed by Deyeux, Trommsdorff, and others, to the presence of mucus, as mucus is said to be the only substance, which is capable of supporting this species of vegetation†. I conceive, however, that this opinion is not correct; and that, even if there be any thing in the infusion, to which the name of mucus properly applies, it is not the immediate cause of the formation of the mould. The muriate of tin, and the solution of jelly are the two principal reagents,

* I employ the term acetate of lead in the restricted sense, in which it is used in the new Pharmacopœia of the London College, where I may remark, the distinction which I pointed out between Goulard and cerussa acetata is recognized, and the appropriate nomenclature adopted. Powell's Translation of the New Pharm; p. 157.

† Ann. de Chem. XVII, 15; and Thomson's Chem. II, 356.

which

which are employed in the analysis of galls, the first being supposed to indicate the presence of the extractive principle, the latter of the tan. The accuracy of this deduction I shall hereafter examine, but admitting it for the present, I may observe, in the first place, that an infusion of galls, which, when recent, was copiously precipitated both by the muriate of tin and by jelly, after it has undergone the process of moulding, will be found no longer capable of being acted upon by the first of these reagents, while the effect of the second is very considerably diminished. Secondly, if successive infusions be formed from the same galls, it is only the first infusions, which are capable of moulding; and it is these only which form a precipitate with the muriate of tin and with jelly. Hence we may conclude, that the capacity of moulding is intimately connected both with that part of the galls which precipitates the muriate of tin, and also, though perhaps in a less degree, with the tan*.

In

* I think it probable, that by proper management, an infusion of galls might, by the operation of moulding, be deprived of all its tan, as well as of what has been called the extract. I kept a quantity of the infusion exposed to the atmosphere for several weeks, and from time to time destroyed the covering of mould as it was produced. Long after the infusion ceased to be affected by the oximuriate of tin, the mould continued to be formed, and the power of affecting jelly obviously decreased, until at length it did no more than produce a degree of turbidness without throwing down a precipitate. At this period, however, the whole of the fluid became so filled with the remains of the mould, and with the sediment which was deposited at the same time, that the experiment could not be pursued. Trommsdorf, as I have noticed above, attributes the formation of the mould to mucus, and even employs this operation to remove this substance, in order to obtain tan in a state of purity. I could not repeat his process, because I was not in possession of any perfectly pure alcohol, which is essential to its success. I would be understood therefore as speaking with much diffidence, when I observe, that I doubt whether it will be found practicable. It goes upon the assumption of the two data, that the extract alone is rendered insoluble by the application of heat and by exposure to the atmosphere, and that the mucus alone is separated by the moulding, both which, according to my experiments, are incorrect. Mr. Deyeux himself has observed (a), that the residue obtained by evaporating the tincture of galls, when dissolved in

Infusion of galls might perhaps be deprived of all its tan by moulding.

Moulding not confined to mucilage.

(a) Ann. Chem. XVII, 16.

Two varieties
of muriate
of tin :

the muriate,

and the oximuriate.

Tests of their
being accurately
formed.

Oximuriate-
preferable re-

In speaking of the muriate of tin it is necessary to observe, that there exist two well known varieties of this salt, which differ, both in the relation of the acid to the metal, and in the state of oxidation of the metal itself. The latter is perhaps the more essential difference, and it is that to which their characteristic effects upon the oximuriate of mercury, and the nitromuriates of gold and platina, are referred. Both the muriates of tin seem to contain an excess of acid, or to be in the state of supermuriates, but it will be sufficient at present to distinguish them by the titles of muriate and oximuriate of tin. The muriate is formed by simply boiling tin in muriatic acid, and preserving it carefully excluded from the atmosphere, and keeping a small quantity of the undissolved metal immersed in the fluid. The oximuriate is procured, either by permitting tin to dissolve in the nitromuriatic acid, or perhaps more accurately, by forming a nitric oxide of tin, and then dissolving this oxide in muriatic acid; this latter is the method that I have generally adopted. In order to ascertain, that the fluids are accurately formed, it is proper to examine their effects upon the oximuriate of mercury, and the nitromuriates of gold and platina; the muriate of tin, in consequence of its strong affinity for oxygen, throws down from the first a gray powder, from the second, what has been called the purple powder of Cassius, and from the platina, a reddish brown precipitate. The oximuriate of tin has no effect upon these solutions. I have not observed, that any difference has been noticed in the effects of these two muriates upon astringent infusions, nor indeed is it stated which of them has been employed*, yet their action is by no means identical. As I have found the muriate of tin a less delicate reagent for the different infusions than the oximuriate, I have employed the

water, is subject to mould; a fact which I have had occasion to notice, and which seems almost incompatible with his opinion of the connection between the mould and mucus. I have also found, that Mr. Hatchett's artificial tan is capable of moulding

* Davy, Proust, and others denominate the substance upon which they operate the muriate of tin; but from the effects which it produced, I apprehend it must have been either what I have styled the oximuriate, or a mixture of the two.

latter

latter in my experiments. In operating with the oximuriate of tin there is a circumstance to be attended to, which may interfere with the results; when the aqueous solution of this salt is very much diluted, it becomes insoluble, and a precipitate is formed, which in experiments on vegetable infusions might be mistaken for the effect of a combination of the oxide of tin with some of the constituents of the substance under examination. The precipitate seems in this case to depend upon the water removing a quantity of superabundant acid, which is necessary to render the salt soluble in water*. Having had occasion to make frequent use of the oximuriate of tin as a reagent, I wished to ascertain what degree of minuteness it possessed as a test for tan or extract, and for this purpose, an infusion was formed by macerating a quantity of finely powdered galls in eight times their weight of cold water for twenty-four hours. Portions of this infusion were successively added to 10, 20, 30, 40, and 50 times their weight of water, and even in the last instance the oximuriate of tin caused a slight precipitate, but no effect could be perceived when the infusion was mixed with 100 times its weight of water. The nitromuriate of tin seems to be nearly as delicate a test, and they are both considerably more so than the simple muriate.

agent for astringent infusions.

but it is precipitated by great dilution.

Its delicacy as a test.

I have had occasion to refer to the effects, that are produced by subjecting the same portion of galls to a number of successive infusions, and I shall now describe these effects a little more fully. A quantity of finely powdered galls was infused in ten times its weight of water, kept at the boiling heat for an hour, and then suffered to stand until the following day, when the fluid was drawn off; the same quantity of water was then added to the residue, which was boiled as before, and the operation was repeated for twelve successive days. This twelfth infusion was colourless, it afforded no precipitate with jelly or the oximuriate of tin, and only a slight gray tinge with the oxisulphate of iron. These infusions were kept for a fortnight, and were then examined. The first infusion contained a large quantity of sediment, and was covered with a thick coating of mould. The 2d

Twelve successive infusions of the same galls at boiling heat.

* Berthollet, Stat. Chim. II, 457.

Successive infusions in cold water.

Effects of reagents.

Gallic acid more readily soluble than tan.

and following infusions, up to the 7th inclusive, were also more or less covered with mould, and had deposited a sediment, but the fluid was now in all of them transparent, and of different shades of brown. The remaining infusions, after the 7th, had undergone no change, their colour was very bright, and in the two last, scarcely perceptible. A comparative experiment was made at the same time with another portion of galls, which was subjected to the same operation, except that it was not boiled, but only suffered to remain for twenty-four hours at the common temperature of the atmosphere. The cold infusions were generally of a deeper brown colour, they continued to act upon the reagents longer than the warm infusions, so that it was not till after the 14th, that the effect of the iron ceased to be visible. Generally the cold infusions begin to mould sooner than the warm ones, but I thought that they deposited less of the sediment. The effects of the three reagents, jelly, the oximuriate of tin, and the oxisulphate of iron, upon the infusions were noticed in every instance when they were first formed; in the earlier infusions the precipitates were very copious, but their quantity gradually diminished, until first, they were no longer produced by the oximuriate of tin, and shortly after by jelly, but it required a considerable number of additional infusions to exhaust the whole of the gallic acid. If the infusions be formed as in the above experiment, it generally happens, that after the 7th or 8th period the oximuriate of tin ceases to produce a precipitate, jelly continues to be perceptible for one or at most two infusions more, while the iron produced the black stain until the 12th, 13th, or 14th infusion.

When the three reagents mentioned above are added to the infusion of galls at different lengths of time after its formation, the iron is the first which produces an effect; while the jelly and the oximuriate of tin commence later, and nearly about the same period. The gallic acid is so readily soluble in water, and it is detected with so much minuteness by the oxisulphate of iron, that almost at the same instant that the galls are added to the water, does the fluid become capable of producing the gallate of iron. I have uniformly found the effects of these reagents to follow this order, although

though it is generally stated, that gallic acid is less soluble than tan*, and it is upon this principle, that Mr. Biggin founded his process for ascertaining the relative proportion of tan and gallic acid in the different substances employed in the formation of leather†.

The solubility of the tan and the extract, so far as their presence is indicated by jelly and the oximuriate of tin, appears to be nearly equal. This is also contrary to the generally received opinion‡, but I ground my position upon the following experiment. A quantity of galls was employed in the state of coarse powder, in order that it might more readily subside from the infusion. The proportion of the galls, and the length of time occupied in the infusion, were gradually diminished, until I found, that by infusing the galls in fifty times their weight of water for only five minutes, a fluid was obtained, capable of forming precipitates with jelly and the oximuriate of tin, which were barely visible, but as far as could be judged by the eye, equal to each other.

Tan and extract nearly equal in solubility.

Beside jelly, the muriate of tin, and the oxisulphate of iron, there have been other agents employed in the analysis of galls. Of these the principal are the sulphuric and muriatic acids, the carbonated fixed alkalis, the aluminous salts, lime water, and the acetate of lead. The acids have been considered as acting principally upon the tan, and with this view have been proposed as a means of separating it from the other ingredients of the infusion§. They are, however, less delicate tests than jelly; for I have found, in the successive infusions, that jelly still throws down a considerable precipitate, when they have ceased to act; of the two, the sulphuric is the more delicate. Nearly the same remarks apply to the carbonated alkalis and to lime water, as to the acids; they both throw down copious precipitates

Other reagents employed.

Acids.

Lime water.

* Seguin and Chaussier, Journ. Polyt. IV, 678.

† Phil. Trans. 1799, 261. Thomson's Fourcroy, III, 93.

‡ Davy, Phil. Trans. 1803, p. 234.

§ The proposal seems to have been first made by Mr. Dizé, but Proust and Vauquelin both agree, that the tan may be completely separated by the acids. Ann. de Chim. XXXV, 37.

from

Aluminous
salts.

Acetate of
lead.

Tartarized an-
timony.

from the infusion of galls*, but in the successive infusions, for some time after they have ceased to act, jelly still continues to produce a precipitate. Lime water has been proposed by Mr. Merat-Guillot as the most commodious agent for separating the tan from the other ingredients in the infusions, in order to obtain it in a pure state†, and Mr. Murray seems to regard it as the least exceptionable process‡. The aluminous salts, alum, the sulphate, and the muriate of alumine, have been employed to denote the presence of extract in the infusions§: but whether they act upon the tan or extract, they are much less delicate in their operation, than either galls or the oxides of tin. By successively diluting an infusion of a known strength, and examining it at different periods with jelly, the oximuriate of tin, and alum, I have always found the effect to cease first in the alum. Sulphate of alumine is rather more delicate than a saturated solution of alum, while muriate of alumine seems to be less so. The most delicate and the most universal precipitant of vegetable infusions is the acetate of lead, which acts equally upon all the constituents, the tan, the extract, and the gallic acid, and removes them completely from the fluid. In the detection of the gallic acid it seems to exhibit even more delicacy than the oxisulphate of iron. Mr. Vauquelin, in an elaborate and valuable paper on the effect of reagents on the different species of cinchona||, employed the tartarized antimony as one of his tests. I formed a saturated solution of it in water, and observed its action on the infusion of galls. The effect is very considerable, converting, as it were, the whole of the fluid into a pulpy mass; the precipitate subsides very slowly, but it is easily separated by a filtre, and leaves the infusion perfectly transparent and

* Deyeux, Ann. de Chim, XVII, 19.

Proust, ibid. XXXV, 32.

Although Mr. Deyeux first noticed the action of the carbonated alkalis upon tan, he does not appear to have attempted to procure it in a state of purity by this process.

† Ann. de Chim. XII, 323.

‡ Chemistry, IV, 275

§ Davy, Phil. Trans. 1803, p. 232.

|| Ann. de Chim. LIX, 113. Journal, vol. XIX, p. 106, 203

colourless,

colourless. To this filtered fluid jelly and the oximuriate of tin were added without the slightest effect, and the oxisulphate of iron only produced a blackish green tinge. In this case it would appear, that the whole of the tan and the extract, and the greatest part of the gallic acid were removed by the antimony. In consequence of the readiness with which the nitromuriate of gold parts with its oxygen, I thought of trying the effect of this reagent on the infusion of galls. Its first effect was to convert the brown colour of the infusion into a dull blackish green, and after some time a brown precipitate was thrown down in moderate quantity. I was led by analogy to try the nitromuriate of platina; the infusion was rendered instantly opaque, and a reddish brown precipitate was formed.

Nitromuriate
of gold,

Constituents
of the soluble
portion of
galls.

These observations on the effect of the different reagents upon the infusion of galls naturally lead to some considerations respecting the constituents of the infusions, and also of the galls themselves. The soluble part of the gall-nut is said to consist of four principal ingredients, tan, gallic acid, extract, and mucus. The distinct existence of each of these substances is supposed to be proved, either by our being able to procure it in a separate state, or by the employment of some tests which may recognize its presence. To the tan and the gallic acid, both these methods of proof are, to a certain extent, applicable; they may, in some degree, be separated from the other parts of the galls, and we are able to ascertain their presence by tests of the greatest delicacy. There is reason to conclude, that, whenever jelly throws down a precipitate from a vegetable infusion, tan is present, and is the immediate cause of the effect; although it is probable, that it is not tan alone which unites itself to the jelly. The existence of gallic acid is most distinctly proved; it may be obtained in a state of almost perfect purity, and it may be detected by the oxisulphate of iron in a way that can scarcely be mistaken. But the proof of the existence of extract is not so direct; it is confessed, that we are unable to procure it separate from the other parts of the galls, and therefore we are obliged to form our opinion, either from the effect of tests, or from the observance of some changes that the infusions undergo, which are thought not

Tan and gallic
acid.

Extract.

to be referable to the other constituents. The circumstances, that have been adduced to prove the existence of extract in the infusion of galls, may be referred to the following heads. 1. When jelly has been added to the infusions, until it no longer produces a precipitate, the fluid will still be precipitated by the oximuriate of tin. 2. If an infusion of galls be exposed for some time to the atmosphere, and especially, if it be kept at an increased temperature, a part of its contents will be rendered insoluble, and will separate from the fluid. 3. If two portions of galls be infused in water, one for a short space of time, and the other for a longer period, they will be found to be differently affected by the reagents; the quick infusion being proportionably more acted upon by jelly, and the slow infusion by the oximuriate of tin. I shall consider each of these points individually, and shall examine how they authorize our conclusions in favour of the existence of extract.

The whole of the precipitate by jelly should be separated, before more jelly is added.

The first of them I do not propose to controvert, and yet I think it presents some degree of ambiguity, of which those who have written or experimented upon the subject do not seem to have been perfectly aware. I am disposed to believe, that the effect has been a good deal exaggerated. When we add jelly to the infusion of galls, it seldom happens, that the whole of the precipitate is separated at once; a part of it remains suspended in the fluid, giving a greater or less degree of opacity; and if in this state more jelly be added, it will appear to produce no more effect, or even by farther diluting the fluid, to render it more transparent, and partially to redissolve the solid contents*. If however, in

* Mr. Davy states, that in the addition of jelly to an infusion of tan, if the jelly be added in excess, part of the precipitated compound will be redissolved. In order to observe this effect the following experiment was tried. A quantity of a weak infusion of galls had about twice as much jelly added to it as I supposed would form the most perfect compound; a dense substance was precipitated, and the whole of the fluid was rendered milky. Two equal quantities of this milky fluid were put into separate glasses, to one an additional portion of jelly was added, and to the other the same bulk of pure water. Both the fluids were rendered more transparent from the effects of dilution, but I did not perceive that it was more so in one case than in the other, although the tan must now have had about ten times its proper quantity of jelly added to it.

this

this case we defer the addition of the jelly, until the fluid shall have had time to deposit its contents in a solid form, we shall find, that a fresh quantity will be precipitated. If this operation be repeated, until the fluid no longer affords any farther precipitate with jelly, the oximuriate of tin will indeed still produce some effect, but not in general a very considerable one. Nor indeed does it certainly follow, that this small quantity of precipitate ought to be attributed to the union of extract with the oxide of tin. Although the infusion has ceased to precipitate upon the addition of jelly, although the jelly was added in small quantities at once, and no more added than what seemed necessary, yet I believe that the fluid may still contain both jelly and tan. I found my opinion upon the circumstance, that in the successive additions of jelly to an astringent infusion, the first quantity added unites with a large proportion of tan, and forms a more insoluble compound, than any of the subsequent ones; and that in proportion as we proceed, the jelly becomes incorporated with less and less tan, and forms a compound less and less insoluble; until at length a substance is formed, which remains in a state of half solution, and which renders the fluid opake, without ever producing a complete precipitate. This kind of combination between jelly and tan may be inferred from some of the experiments, which I mentioned in my former paper, and will be farther supported by the following considerations. The weight of the precipitate formed by the addition of jelly to an astringent infusion is considerably influenced by the manner in which the jelly is added, whether all at once, or in successive portions. If we add together at one time the proportion of tan and jelly which we suppose will mutually saturate each other, we procure a dense precipitate, which separates immediately, and leaves the fluid transparent; whereas if jelly be added to tan in successive portions, a larger quantity is necessary before the fluid exhibits an excess of jelly, the precipitate separates more slowly, it is in larger quantity, less dense in its consistence, the fluid retains a degree of opacity, and continues for a considerable time to deposit a sediment. The following comparative experiment bears also upon the same point. An equal quantity of the extract of rhatany and of prepared jelly, each in solution, were

Compound of
jelly and tan
less and less
insoluble,

and differs according to the
mode in which
the jelly is
added.

Experiments
with rhatany.

added

added together, and a dense precipitate was formed, the fluid was left transparent, and nearly in a neutral state. The same quantity of jelly as before was then added to $\frac{2}{3}$ of the former quantity of rhatany; here a precipitate was thrown down, which was less dense and of a lighter colour, the fluid was rather less clear than in the former case, and it produced a slight precipitate by the addition of more rhatany. A third experiment was then performed the reverse of the last. The original quantity of rhatany was added to $\frac{2}{3}$ of its weight of jelly; the fluid was rendered perfectly opaque, but the precipitate very slowly subsided, and it continued for several days to deposit fresh quantities of sediment, but no farther precipitate was produced by the farther addition of jelly. Beside this imperfect compound of tan and jelly, which I suppose to be still retained in the fluid, it certainly contains gallic acid, and probably the neutral and earthy salts, which are found in the gall-nut. I am not prepared to say, that it is upon any of these substances that the oxide of tin acts; but I think, that we are justified in hesitating before we conclude, that the precipitate which is formed depends upon a substance, the existence of which is only rendered evident by the process in question.

Precipitation
by exposure to
the atmos-
phere and heat
questionable
proof of the
presence of
extract.

The second proof, that has been brought of the existence of extract in the infusion of galls, is the circumstance of a part of the matter in solution being rendered insoluble by exposure to the atmosphere, or by the application of heat, a property which is thought to be characteristic of this peculiar substance. Here again, without disputing the fact, I am inclined to hesitate as to the inference, and to doubt, whether the matter which separates be confined to this peculiar constituent of the fluid. If the infusion of galls be evaporated by a heat not greater than that of boiling water, a deep brown, brittle, transparent mass will be obtained, which cannot be entirely redissolved; and if the clear part of the solution be poured off, and treated in the same manner, an insoluble part will again be obtained; and this operation may be repeated for several times in succession on the same portion of fluid with the same result. I have carried it to the fourth period, and I have observed no change in the nature of the fluid, nor did its power of producing

ducing the insoluble residue appear to be diminished. This experiment proves, that the supposed extract of an astringent infusion cannot, according to the common opinion, be separated by one evaporation; and I think it may lead us to doubt, whether the effect is not rather produced upon its solid contents in general, than upon any one part of them. In confirmation of this supposition we may observe, that there are several processes, in which the tan itself is rendered insoluble by the addition of oxygen. Such is thought to be the method, in which the oxide of tin operates upon it; for although what is thrown down is a compound of tan and oxide, yet if the oxide be removed by the action of a hydrosulphuret, the tan still remains insoluble. The same kind of effect is also produced by the nitric and the oximuriatic acids, they throw down from the infusion of galls an insoluble compound, and deprive the fluid of the property of precipitating jelly. The idea, that insolubility after evaporation was a specific characteristic of extract, seems to have originated from the experiments that were performed by Mr. Fourcroy on the bark of St. Domingo; but the constituents of this bark are so different from those of galls, that we are not authorized in extending the analogy from one to the other, unless it be supported by some independent facts. So far therefore as I may be warranted to draw any conclusion on this subject from my own observation, either of the effect of successively evaporating the same infusion, or of the changes mentioned above, which are produced by long exposure to the atmosphere, I should conclude, that the tan itself is rendered insoluble in both these operations.

The third circumstance, which has been adduced to prove the existence of extract, and especially to distinguish it from tan, is the greater insolubility of the latter while they both exist in their natural state. In analysing vegetable astringents we are told, that by subjecting them to a hasty infusion we shall procure the tan nearly free from extract, while by continuing the infusion for a greater length of time we get a fluid which chiefly consists of this substance. It is true, that in applying water to galls, the first portion takes up more than the subsequent ones; but that

Not distinguished from tan by its greater solubility.

which

which is afterwards taken up does not seem to be materially different from what is first dissolved; it only appears, that in this, as in every other instance, the latter portions of soluble matter are retained more obstinately by the insoluble part. The relative effect of jelly and the oximuriate of tin were always, as far as I could judge, exactly in proportion to the strength of the infusion, whether it formed in a longer or shorter time; in the infusions which were made the most hastily, both the reagents produced a precipitate, and however long the maceration had been continued, still the effects seemed to be proportionate to each other. Indeed the results which are obtained, when we make a number of successive infusions, are directly adverse to the commonly received opinion; for I found, as I have already stated, that in the last infusions jelly was frequently capable of forming a precipitate after the oximuriate of tin, but that the converse never took place.

Farther difficulty in distinguishing tan from extract.

It may be farther observed, respecting the distinction between tan and extract, that the two reagents, which are the appropriate tests of each, jelly and the oximuriate of tin, both of them act powerfully upon the opposite substance, i. e. jelly upon extract, and the oximuriate of tin upon tan. With respect to the latter, it is known that Proust, who has exhibited so much sagacity on the subject of vegetable infusions and their action upon the metallic oxides, originally introduced the muriate of tin as a reagent for tan, and in his first experiments seems to have had no idea of its acting upon any other substance*. And with respect to the effect of jelly, whatever may be the body upon which it exerts its primary action, we find, that, when it has ceased to precipitate an infusion, what is then thrown down by the oxide of tin is at least in very small proportion to what would have been produced in the recent infusion. The facts which I have mentioned above, respecting the successive infusions and the formation of mould show an intimate connexion between the two supposed substances, and indeed scarcely permit us to draw any line of distinction. And the same idea will be still farther countenanced by con-

* Ann. de Chim. XXV, 225.

sidering what are the characteristic properties of extract, as stated at full length by Mr. Vauquelin; for we shall find, that, so far from marking any essential difference between this substance and tan, they are equally applicable to the latter, and have been pointed out as even peculiar to it. Among these we may notice its solubility in water and alcohol*, its strong taste, the effect of oximuriatic, nitric, sulphuric, and muriatic acids, of alkalis, and of metallic oxides†.

Are we then to conclude, that the infusion of galls does not contain any constituent, to which the title of extract ought to be applied? or that, according to the original opinion of Proust, Seguin, and others, the infusion consists merely of tan and gallic acid? Upon this point I do not feel myself qualified to give a decisive opinion. Although I think the proofs, that have been adduced in favour of the existence of extract, are very insufficient, that we are not yet in possession of any method of accurately recognizing its presence, and that we are unable to say what are its characteristic properties; yet I do not conceive, that we are warranted in denying its existence. There is indeed one fact which seems a strong presumption in its favour, viz. that, if we take two portions of the same infusion of galls, and saturate one of them with jelly, and the other with the oximuriate of tin, and let each of them remain for some time exposed to the atmosphere, until they have deposited all their precipitates, they will then each of them afford

Does the infusion contain nothing but gallic acid and tan?

Fact in favour of the existence of extract:

* Tan is insoluble in perfectly pure alcohol, but it is readily dissolved in the alcohol with which our experiments are usually performed.

† It is stated, that extract is insoluble in ether; but this does not apply to that part of the infusion of galls, which is acted on by the oximuriate of tin, for this reagent forms a copious precipitate with a solution of the substance which is left by evaporating ether that has been digested on galls. This substance appears indeed to act as readily upon the oximuriate of tin as upon jelly or gallic acid, and I could not perceive, that it differed in any respect from the substance procured from the aqueous infusion of galls, except in being lighter coloured. Mr. Murray observes (a), that the colouring matter of saffron, which has been regarded as a specimen of pure extract, is readily soluble in ether.

(a) Chemistry, IV, 264.

some

but this far
from decisive.

some precipitate to the contrary substance, the fluid which had been saturated with jelly will be precipitated by oximuriate of tin, and the fluid which had been saturated with the oximuriate of tin will be precipitated by jelly. Yet when we consider the compound nature of the fluid upon which we operate, and the variety of actions which may take place between the different reagents, we are not authorized even from this experiment to draw a decided inference in favour of the existence of two distinct substances. In entertaining doubts respecting the existence of extract as a distinct principle of vegetables, I feel happy to have my opinion supported by that of Mr. Murray*.

(To be concluded in our next.)

X.

Question on the Preparation of Cork for Modelling. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

How may the
elasticity of
cork be de-
stroyed?

I Should be obliged to you, or any of your correspondents, if they could inform me of the method of destroying elasticity in cork; or what process it undergoes, to render it fit for modelling. If you recollect its having been noticed in your Journal, by mentioning the volume you will equally oblige,

Yours, &c.

R. Z. A.

ANSWER.

Elasticity of
cork owing to
its texture,

I am not acquainted with the method of depriving cork of its elasticity, but do not think my correspondent will find much difficulty in discovering it by experiment.—From the texture of cork as seen under the microscope, it appears to

* Chemistry, IV, 260.

be formed of woody fibres, with large interstices between them; and the elasticity of this substance is probably owing to the flexibility and spring of these fibres, and the difficulty with which the included air can be driven out from the cavities. It should seem, that, if the fibres could be rendered less flexible, and the spaces partly filled, the whole mass would become much less elastic. This may be tried by experiments on a small scale. A slice of cork may be immersed in any hot liquid, which becomes stiff or brittle by cold, such as melted resin, or its solution in alcohol, or glue, or gum water, or tallow, or starch, or varnish, or any other material having the property first mentioned, and of which the list is not very numerous. The cork should be repeatedly compressed under the fluid, in order that it may imbibe it, and the whole allowed to cool before the cork is suffered to rise from the surface. Many trials of this sort may be made in a short time over a candle in an iron spoon, or in the small copper or iron vessels used for pastry; and when the result is thus obtained, the operator must contrive and manage a larger apparatus according to his convenience, and the intended purpose.

W. N.

XI.

On the Dusodile, a new Species of Mineral; by Mr. L. CORDIER.*

THE new bituminous substance, which I am about to make known, was found in Sicily by Dolomieu. The specimens collected by that celebrated mineralogist arrived at Paris about ten years ago; and I then drew up a description of it under his eye, but various circumstances had prevented me from publishing it. I shall now give it, adopting the method of Haüy.

A species of
bitumen found
in Sicily.

* Journal de Physique, vol. LXVII, p. 277.

The

Its state.

The dusodile is in the compact state, and presents itself in the form of irregular masses, which fall into very thin leaves with great facility. The following are its characteristics.

Essential character.

It burns with an extremely strong and fetid bituminous smell; leaving a considerable earthy residuum.

Physical characters.

Its specific gravity is 1.146.

With respect to hardness, it is easily cut, and reduced into thin and very fragile leaves.

The leaves are a little flexible.

Its colour is greenish gray.

It is opaque; but the thin leaves become translucent by maceration in water.

Its smell, when breathed upon, is argillaceous.

Chemical characters.

It is weakly combustible with a clear flame, and an insupportable bituminous smell, resembling that elicited by friction from the most fetid calcareous stones. This smell is so strong, that we are not very sensibly affected by it till a few instants after the combustion, that is to say, when the smoke is completely diluted with the air. The burning of a very small piece is sufficient to poison a room for more than an hour.

Combustion leaves a considerable earthy residuum, forming more than a third of the original weight.

By maceration in water its leaves separate of themselves, and become not only translucent, but perfectly flexible.

Distinguishing characters.

Dusodile is distinguishable from coal, by the latter being always of a black colour, more dense, and not changed by the action of water. From bitumens, whether solid or glutinous, as these, when heated gently, or rubbed between the fingers, emit a smell resembling that of pitch; and when burned leave scarcely any earthy residuum, and give out no such smell as the dusodile. From the common elastic bitumen, as this has naturally a very perceptible bituminous smell, and is completely elastic; while the dusodile is in very fragile leaves, and emits an argillaceous smell when breathed on. The elastic bitumen too burns with leaving scarcely any residuum, and emitting a smell that is neither powerful nor disagreeable. From indurated elastic bitumen,

bitumen, as this burns in the same manner as the preceding, its fragments exhibit no appearance of flexibility, and maceration in water does not in any respect alter their consistency.

Its texture admits of no variety, being at the same time compact and foliaceous: but it has two varieties of colour, greenish gray, and yellowish gray.

This mineral is found at Melilli, near Syracuse. It forms a stratum of no great thickness, extended between beds of secondary limestone.

It appears, that attempts have been made to work it out, but they have not been pursued. This is certain, that the combustible fossil it contains has long been known in the country. The inhabitants give it different names, some calling it the bituminous foliaceous earth of Melilli, others devil's dung. Both these names being equally improper, I have thought it necessary to frame one more suitable to mineralogical nomenclature. That of *dusodile*, which from its Greek root implies fetid, was naturally suggested by one of the most remarkable properties of this new kind of bitumen, that of diffusing a detestable smell when burned.

XII.

*Memoir on the triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. LE COMTE DE BOURNON, F. R. & L. S.**

THIS memoir was written chiefly as an answer to that printed in the first part of the Philosophical Transactions for 1808 †, in which Mr. Smithson its author, criticises with as little justice as decency a former memoir of

Former memoir of the author criticised by Mr. Smithson.

* Translated from the original, communicated by the author, and revised by him.

† See Journal, vol. XX, p. 332.

mine on the endellion, which was printed in the first part of the Transactions for 1804. It may appear strange, particularly to those who have read Mr. Smithson's sharp critique, that I have so long delayed answering it: but this delay was owing to one of those peculiar circumstances, happily not very frequent, which the mind is as unable to foresee, as prudence is to avoid. Chance made me acquainted with the criticism of Mr. Smithson, at the time of its being delivered to the secretary of the Royal Society, Dr. Wollaston, at whose house I then happened to be. He gave me permission to look it over. Its nature surprised me; and this was all the impression it would have made on me, had I not immediately felt the disagreeable necessity I should be under of answering it, if it should be admitted into the Transactions of the Royal Society; a circumstance, which I could never have supposed would take place, had I not had some particular reasons to be apprehensive of it. I requested in consequence Dr. Wollaston to favour me with a copy as soon as it should be printed; which he promised me. Some time after, being at the house of the same gentleman, whom I was frequently led to visit by the esteem and attachment I felt, I found on his table the corrected proofs of this very paper, and then reminded him of the promise he had made. In fact he sent me a copy soon after. Happily I had in readiness the materials necessary to render my answer in some degree interesting, and prevent my feeling the irksomeness commonly attendant on writings of this kind. After I had written my former paper, I had obtained a knowledge of this substance, at that time extremely scarce, and considerably so even at present, that enabled me to render my account of it far more complete: and this indeed I had for some time had an intention of doing. A pursuit however in which I was then engaged, and which I could not interrupt, did not allow me immediately to draw up the memoir I had in contemplation: and it was not till about the month of September, in the same year, 1808, that I was able to deliver it to the secretary of the Royal Society; expressing at the same time my wish, that it might be read as early as possible, in order that at least it might obtain a place in the first part of the Transactions

Farther knowledge of the compound sulphuret obtained by the author.

Account of it delivered to the secretary of the Royal Society.

Transactions for 1809. This I had the greater reason to hope, as the time when it was delivered was considerably before that, when the Royal Society recommenced its meetings. After these took place however, I found it impossible to get it read, notwithstanding I requested it repeatedly. The time passed on, and I could not but foresee, that it would have a powerful opposition to surmount in the committee of the Royal Society, under which it would probably sink. I could not however make any other use of the paper. The first part of the Transactions was already filled up, when at length I learned from Dr. Wollaston, that it had been read on the 4th of May. The time however still passed on, and nothing gave me reason to suppose, that the committee was taking any steps to have it printed. On this subject it preserved the profoundest silence, which I endeavoured to penetrate in vain. It was not till the 23d of June, when its vacation was nearly approaching, that I was informed of the fate, to which apparently it had been condemned from the beginning, by a letter from the committee, in which it was said, that, not deeming it expedient to print it at present, it was ordered to be deposited in the archives of the Royal Society.

Ordered to be placed in the archives of the Society :

Such are the reasons, that have hitherto prevented the publication of this paper, and at the same time have deprived it of the place it ought to have occupied. In fact it seems, that, since the critique of Mr. Smithson, as unbecoming as it was unfounded, obtained a place in the Transactions of the Royal Society, it could not without injustice refuse one of its members, whom it must have seen with regret to be the object of it, and who hitherto had been a zealous and approved coadjutor in its labours, the only indemnification he could receive, that of demonstrating the truth of his first assertions. I appeal to the Members of the Society themselves, who shall read this paper, and who know me sufficiently to do me the justice, I think, I deserve, whether I could on any account have expected this singular proceeding on the part of its committee.

but should have been published.

ENDELLION.

Part I.

Endellion not a simple ore of antimony, When in the month of December, 1803, I presented to the Royal Society a paper on the triple sulphuret of lead, copper, and antimony, considered at that time as a simple ore of antimony, I thought it the more necessary, to fix the external characters of this substance, as Mr. Hatchett had just shown by his analysis of it, that, so far from being a simple ore of antimony, it was an ore composed of three sulphurets, those of lead, copper, and antimony, in which the latter was not even the principal metal.

but a triple sulphuret.

The character of the crystallizations could not at first be sufficiently established. It was not in my power at that time however, to establish the character of the crystallizations of this triple sulphuret with all the precision, that the case required, and that I could have wished. This substance was then extremely scarce, as it is even now. The crystals I was able to procure being small, and with numerous sides, most of which were irregular, did not allow me to depend sufficiently on the measures I was able to take, to venture to fix in a peremptory manner the dimensions of its primitive crystal. All that I could then determine positively was, that this crystal was a rectangular tetraedral prism with square bases, but not a cube. In consequence I satisfied myself with establishing this truth, without settling the dimensions of the crystal. It necessarily followed, that the measures given as those of the angles of incidence between the primary and secondary faces, as they could not be the result of a calculation the base of which was not determined, must have been merely taken with the graphometer, and consequently to be considered as approximations only; yet as approximations having all the accuracy the instrument would admit, and the inaccuracy of which could not amount to one degree.

Primitive crystal.

Desirous from that period of giving a more complete account of this rare and interesting substance, and ascertaining at the same time in a more positive manner the form of its primitive crystal, I omitted no opportunity offered me of continuing to study it. For such opportunities, as I could

could not make them, I was under the necessity of waiting. At length I obtained the object I wished; I met with crystals, that I could measure with certainty, and had the satisfaction to find, not only that I could lay before the Royal Society more precise observations respecting the character of the crystallization of this substance, but besides a more complete and interesting series of its varieties of form, and a much more complete mineralogical account of every thing concerning it. I had at the same time the satisfaction to find, that the first measures I gave, which were simply taken from the crystals with the instrument, differed from those now established by calculation only in that slight degree, which may be ascribed to the unavoidable want of accuracy in the instrument; a difference amounting only to 30' in one of the three varieties I formerly gave, to 19' in a second, and to nothing at all in the third. This fact may serve as a proof of the near approach to accuracy obtainable by a little practice in using the instrument alone*.

More crystals
of it obtained.

I shall now proceed to give a complete account of this substance, pursuing the method I adopted in my treatise on mineralogy, the first two volumes of which are just published.

By way of preliminary however I shall observe, that as all substances, beside the explanatory terms that point out their nature, and which are liable to change with the theory on which they are founded, require a proper name, invariable in itself, and fixing their existence among natural substances, I have given this the name of *endellion*; which avoids the termination in *ite*, so frequent in the nomenclature of mineral substances, and calls to mind, that the first specimens of this substance, which engaged the attention of mineralogists, came from Endellion, in Cornwall.

All substances
require an appropriate
name.

This named
endellion by
the author:

At the same time I avail myself of this opportunity, to

bournonite by
Jameson.

* *Additional note.* Since this paper was written, the measures obtainable by the instrument have acquired much greater precision by Dr. Wollaston's ingenious discovery of the reflective goniometer, a discovery of great importance to crystallography.

testify

testify to Mr. Jameson how sensible I am of the flattering but unmerited honour he has paid me in his mineralogy, by giving this substance the name of bournonite.

Its characters.

ESSENTIAL CHARACTERS OF ENDELLION.

Crystallographical.

Primitive crystal. *Primitive crystal.* A rectangular tetraedral prism, the bases of which are square, and the height of which is to the side of the terminal faces in the proportion of 3 to 5.

Integrand molecule. *Integrand molecule.* I have yet met with nothing to induce me to adopt any particular opinion of the form, that belongs to the integrand molecule of this substance.

Fracture. *Fracture.* Irregular, and partially conchoidal. Made on crystals, and extending but a little way, it is perfectly conchoidal. Its lustre is very brilliant. Some of its accidental fractures exhibit more clearly than many of those made by art the direction of its laminae parallel to the faces of its primitive tetraedral prism; but these traces are always faint, and seldom perfectly marked.

Physical.

Spec. gravity. *Specific gravity.* 5.775.

Brittleness. *Fragility.* This substance is very brittle. It is easily broken by the simple pressure of the nail.

Hardness. *Hardness.* The endellion scratches carbonate of lime with considerable facility, but not without breaking, on account of its great brittleness. Rubbed on paper it leaves a blackish brown mark. Its powder retains the metallic lustre.

Colour. *Colour.* Dark gray, and very shining, like that of polished steel, but a little more dark.

Chemical.

Very fusible. Exposed to the action of the blowpipe, it fuses the instant it is touched by the flame. It remains for some instants afterward in a fluid state; and, if it were fused in a spoon, it might be cast like melted lead, the fluidity of which in this state it even surpasses. A metallic regulus is easily obtained from it, of a dark gray colour, very brittle,

Regulus.

brittle, and the fracture is of a grain very fine and smooth.

Thrown into cold nitric acid, it dissolves pretty readily, and with effervescence. A real analysis is thus accomplished. The sulphur swims on the liquid, which holds in solution the copper and lead, and is of a green colour, and the oxide of antimony is precipitated in the form of a blue powder inclined to gray. Action of nitric acid on it.

The endellion analysed by Mr. Hatchett gave for its component parts sulphur 17, antimony 24.23, lead 42.62, copper 12.8, iron 1.2: loss 2.15. Component parts.

Eventual characters.

Phosphorescence. Placed on a hot iron the moment it begins to lose its red colour, the endellion diffuses a blueish white phosphorescent light, the intensity of which appeared to me to vary in different specimens. Phosphorescent.

TABLE of the ENDELLION and its VARIETIES.

<i>Species.</i>	<i>Varieties.</i>	<i>Varieties.</i>
Triple sulphuret of lead, copper, and antimony. Endellion.	Crystallized in a perfectly determinate manner.	Primitive crystal. Its modifications and varieties.
	In the compact state.	Pure. Mixed irregularly with sulphuret of zinc. Mixed irregularly with yellow sulphuret of copper and iron.

DESCRIPTION of the DIFFERENT VARIETIES of the ENDELLION. Description.

Of a determined crystalline form.

The perfectly determined crystalline form is that in which this substance has hitherto most commonly occurred. The surface of its crystals has a very shining lustre, which can be better compared to nothing than to the rhomboidal oxide of iron of the island of Elba, oligist iron of Haüy. This lustre however is exceeded by that of their fracture, when

when it is recent. The crystals are difficult to determine, both on account of the great number of faces they frequently exhibit, and of the irregularity of these faces. They consequently require a great deal of attention on the part of the observer, to be perfectly ascertained.

Where found. The endellion in a state of completely determined crystallization was first observed in Cornwall, in the mine of Huel-boys, in the parish of Endellion; and from this mine have been obtained the finest groupings of this substance that are seen in collections, where it is in general very rare. The endellion exists likewise in Siberia, where too it appears to be very scarce, as I know of but a single specimen, which is in my possession. I have seen in the shop of Mr. Maw several fragments of this substance, sent with other minerals from Brazil, the biggest of which was a very large single crystal, of the variety represented pl. vii,* fig. 8. Lastly I am indebted to Dr. Wollaston for some small fragments of the same substance, in which the endellion is in the compact state, mixed irregularly and very visibly to the eye with the double yellow sulphuret of copper and iron; and in which are observable little cavities, including very small but well defined crystals of the same substance, intermixed with minute specks of carbonate of lime and sulphate of barytes. These fragments came from Peru. The endellion, which I mentioned above as coming from Brasil, is in like manner mixed very perceptibly to the eye with yellow sulphuret of copper and iron.

The groupings of crystals of endellion from Cornwall are frequently accompanied with crystals of brown sulphuret of zinc; and in several sulphuret of antimony is likewise observed, commonly in fine needles, and frequently even capillary.

In the compact state.

In the compact state.

The same pieces, that include crystals of this substance in Cornwall, include also parts more or less large, in which it is in the compact state. When this occurs, its fracture is

* The plates to this article are unavoidably deferred to our next number.

thus rendered irregular and granular, and its lustre is much inferior to that of the fracture of the crystals.

This compact variety in Cornwall is frequently mingled with sulphuret of zinc, which may easily lead to mistakes; and which Mr. Hatchett has very judiciously noticed in the analysis he made of this substance.

The compact endellion of Brasil and Peru also is intimately mixed with yellow sulphuret of copper and iron.

Description of the crystalline forms of endellion, and observations respecting them.

The primitive crystal of this substance, as I have already said, is a rectangular tetraedral prism, pl. vii, fig. 1, the height or side of which is to the sides of its terminal faces in the ratio of three to five*. I have not yet seen this crystal in its perfect state, that is to say, without the planes of any of the modifications belonging to it; and it cannot be obtained by splitting, the attraction of cohesion, that unites the integrant molecules of this substance, being too strong to be overcome. By means of some of the accidental fractures however, that occasionally exist, I have been able to discover the direction of its laminae, and perceive that this direction, as well as that of the secondary faces, agree perfectly with the results of calculation.

I do not think however, that this prism is at the same time the form of the integrant molecule of this substance: but hitherto nothing has led me to form any particular opinion with respect to the form of this molecule.

As the crystals of endellion are frequently loaded with facets, which the mineralogist may find embarrassing, I have thought it necessary, for the ease of the reader, to give separately, in fig. 4, the various retrogradations† experienced by the laminae of the crystallization, which I have observed

* The method I have pursued for the determination of the primitive crystal will be seen hereafter.

† I give the name of retrogradation [*reculement*] to that act of crystallization, which has hitherto been known by the name of *decrement*, an expression that is totally false in many cases, as I have shown in the second volume of my Treatise on Mineralogy, p. 206, in the part relating to the theory of crystallization.

on the longitudinal edges of the primitive prism; in fig. 5, those I have observed along the edges of the terminal faces; and in fig. 6, those that I have observed at the angles of these faces. These three figures are intended for the same purpose of convenience, as those which, in my former paper on this substance*, were given solely with this view, and the exact models of which had not yet been observed in nature. My experience in crystallography has frequently led me to remark, that, when the crystals of a substance are liable to any considerable number of modifications, and at the same time actually undergo several of them, this method is extremely useful, and frees the mineralogist, who is desirous of ascertaining one of these crystals, from a task not unfrequently very troublesome. There are even substances, in which this method is very advantageous to the most expert crystallographer, and the present is one of them.

1st modifica-
tion.

1st modification. The planes arising from this modification substitute for the longitudinal edges of the primitive prism a plane equally inclined to those contiguous to it. They are produced by the retrogradation of one row of the particles of the laminæ along these edges. These new planes are frequently striated, as is shown in fig. 2. Sometimes they cause the complete disappearance of the faces of the primitive crystal, giving rise to another prism, which is likewise a rectangular tetraedron with square bases, but secondary to the primitive prism; and in the crystals of this variety that I have seen the faces were constantly striated, as in fig. 3. This variety even led me into a mistake, when I wrote the first paper on this substance I presented to the Royal Society, by inducing me to consider the planes of the prism of the varieties represented at figs. 10, 11, 15, 16, and 17, as belonging to them: but a more attentive examination, elucidated by observations since made on a great number of other crystals, has taught me, that these striæ were the simple effect of aggregation, and that these same planes belonged to their primitives†.

I shall

* *Additional note.* — and for which I was so unhandsomely re-proved by Mr. Smithson, in his critique printed in the Philosophical Transactions.

† The striæ, that occur so frequently on the planes of crystals, are often

I shall not enter into any similar details concerning the other modifications, the number of each of these being affixed in each crystal to the planes belonging to it, and the table of these modifications, which will be annexed to this paper, pointing out in this respect at a single view particulars, which could scarcely be expressed by long circumlocution. In consequence I shall confine myself to the following observations.

All the varieties, from fig. 2 to fig. 20 inclusively, are met with among the fragments of this substance, that are brought from Cornwall. I have a very fine groupe of those represented at figs. 7 and 8, and a separate crystal of 8 and of 9. The variety fig. 8 is a regular aggregation, in the form of a cross, of two of the crystals fig. 7 elongated parallel to the planes of the 6th modification. It might also, and perhaps more justly, be considered as resulting from five crystals, similar to fig. 7, united by one of their planes. I have likewise crystals of the varieties 11, 12, 14, 15, and 19. With those at figs. 10, 13, 16, 17, and 18, I was furnished by two very fine groupes, and a superb single crystal, in the possession of Mr. R. Phillips. Fig. 16 answers to that numbered 17 in my former paper, which was not quite accurate. In 15 and 16 of the same paper the prism was much too thick, and they are replaced at present by 17 and 18.

I have likewise the varieties from 21 to 26, in a very fine group, which, with the fragments from Peru and Brasil often of very great use to indicate the direction of the laminæ of crystallization; and not seldom are they the only means, that the crystals of a substance afford. Thus the lenticular rhomboidal carbonate of lime pretty constantly indicates by its striæ the direction of the laminæ, and consequently that of the planes of the primitive crystal. In the hexaëdral prism of the same substance, the same striæ point out the direction to be given to the fractures, on which Mr. Haüy has established the dimensions of its primitive rhomboid. But we must beware of the illusion, that may sometimes arise from striæ, which are indebted for their existence only to an aggregation of crystals, such frequent instances of which are exhibited by the tourmalin, thallite, sulphuret of antimony, &c. : an illusion by which I have shown I was at first misled myself with respect to the endellion, after having observed, that among its varieties there existed a rectangular tetraëdral prism, the planes of which are frequently striated.

ready

ready mentioned, constitutes the only specimens I have yet seen from any place except Cornwall. The groupe, which assuredly is not English, was given to me as coming from Siberia. Its gangue is an irregularly crystallized quartz, part of which is of a dark blackish gray, in consequence of a mixture of very minute particles of sulphuret of lead, imperceptible to the naked eye. The crystals of endellion are covered by a slight stratum of green carbonate of copper; and some small crystals of common dodecaedral pyramidal carbonate of lime (*métastatique* of Haüy) are disseminated among them as well as in the quartz. Several small parcels of sulphuret of lead and blue copper are likewise observable on it.

Probably other
varieties.

Such is the result, which the most careful examination, and continual attention to every thing, that could render me better acquainted with this scarce and interesting substance, enables me at present to lay before the Royal Society. I am far however from imagining, that I have seen every thing pertaining to its crystallization. Undoubtedly other varieties, and other modifications, may exist; and it is probable, that, among the small number of specimens of it in different collections, such may be found. From the numerous varieties, that exist in a single groupe of this substance, its primitive crystal appears to have a great tendency to be modified: but the modifications of this crystal, which I have given, are unquestionably sufficient, to render it easy to ascertain any new ones, if they should occur. These reflexions are not introduced here without reason. Among the different specimens of this substance examined by me, I have seen several crystals belonging to some of the varieties I have given, on which there existed likewise slight traces of planes belonging to other modifications, but which it was altogether impossible for me to determine. As an example of this I shall mention the crystal represented at Pl. VIII, fig. 27, not only because it is one of the most striking for elegance of form, but because it is in my own possession. The faces indicated by the letters *x*, *y*, and *z*, are certainly owing to an intermediate retrogradation at the angles of the terminal faces: but the impossibility of measuring with precision the angle of inclination between these planes
and

the primitive ones in this crystal, which is very small, and partly imbedded in its gangue, completely prevents me from determining the nature of the three different retrogradations, to which they owe their existence.

(To be continued in our next.)

XIII.

On Detonating Silver. By Mr. DESCOTILS.*

MR. Figuier, prof. of chemistry at the Pharmaceutical School at Montpellier, has lately written to the authors of this collection a paper on detonating silver, in which, after mentioning that Mr. Howard first formed this compound, which was afterward obtained in larger quantity by Mr. Cruickshank, he points out a process for preparing it analogous to that adopted by the latter gentleman.

A paper already published in this Journal† contains nearly similar results to those obtained by the professor of Montpellier, we shall therefore confine ourselves to the differences mentioned in his observations.

Mr. Figuier has seen the detonating silver explode even amid the acid solution in which it is formed, when touched by a hard body. He has likewise detonated this compound when dry by simple friction with the edge of a card. These facts indicate a much greater degree of inflammability than had been supposed, and must lead us to be more cautious in preparing this substance.

The professor has remarked, that detonating silver is not decomposed by weak sulphuric acid, unless it has been previously dissolved in water.

Caustic potash appeared to him merely to change its colour to a red, or a deep gray, without depriving it of its fulminating quality. This experiment, which I repeated, did not afford me precisely the same result. After remaining a considerable time in potash, the residuum gave only a slight decrepitation, arising no doubt from the portions, on which the potash had not yet acted.

* Annales de Chimie, vol LXIII, p. 104.

† Journal, vol. XVIII, p. 140.

Fulminating silver.

Former paper on the subject.

It explodes more readily than is usually supposed.

Not decomposed by weak sulphuric acid, unless in solution. Nor by pure potash.

XIV.

XIV.

Process for making a fine Lake.*

Fine lake

A German chemist, whose name is not mentioned, has published the following process for making a beautiful lake.

by precipitat-
ing cochineal
with solution
of tin.

Take any quantity of cochineal, on which pour twice its weight of alcohol, and as much distilled water. Infuse for some days near a gentle fire, and then filter. To the filtered liquor add a few drops of solution of tin, and a fine red precipitate will be formed: Continue to add a little solution of tin every two hours, till the whole of the colouring matter is precipitated. Lastly, edulcorate the precipitate by washing it in a large quantity of distilled water, and then dry it.

XIV.

On the Blue Wolfbane, by PHILIP ANTONY STEINACHER.*

Blue wolfbane
contains green
fecula,

THE fresh leaves of blue wolfbane, *aconitum napellus*, cultivated in a garden near Paris, being treated with a sufficient quantity of water at 45° [113° F.], green fecula was coagulated.

a gas,

The liquor separated from this fecula retained a peculiar herbaceous smell, analogous to that of the leaves of scurvy grass after the greater part of their pungency is destroyed by exposure to the open air. The progress of evaporation entirely dissipated it. Toward the end a matter of a granular form was separated. After this was washed and dried, a portion subjected to the action of the blowpipe on platina was not melted by the interior flame, but became whitish, without swelling or decrepitating.

an earthy mat-
ter, consisting
of

carbonate

and phosphate
of lime,

Another portion put into weak sulphuric acid produced a pretty long effervescence. The evaporation of the fluid afforded acidulous crystals in the form of soft needles, which were decomposed by nitrate of lead. The precipitate, heated red hot by the blowpipe on a piece of charcoal,

* Sonnini's Bibliothèque physico-économique, for 1808, vol. I, p. 352.

† Journal de Physique, vol. LXVI, p. 234.

was reduced into little metallic globules, round which a slight aureola shone, accompanied with a very perceptible phosphoric smell. The extractive liquor boiled down contained a great deal of ammoniacal muriate. and muriate of ammonia.

As other plants gathered by the side of the wolfsbane yielded me no signs of phosphate when analysed, I conceive the organs of this plant have the faculty of assimilating phosphorus, or its elements, and converting them into an acid. No phosphate in other plants growing near it.

From my analysis it follows, that the *aconitum napellus* contains Summary of its contents.

Green fecula,

An odorant gaseous substance, which I suspect to be virulent,

Muriate of ammonia,

Carbonate of lime, and

Phosphate of lime.

Thus the existence of this phosphate in the blue wolfsbane, which Mr. Tutton of Wolfenbüttel announced nineteen years ago, is confirmed. The phosphate observed formerly.

SCIENTIFIC NEWS.

THE annual courses of popular lectures at the Surry Institution, Blackfriars Bridge, commenced on the 31st ult., and will be continued every succeeding Tuesday and Thursday evening, at seven o'clock, during the season. We understand, that the following gentlemen have been engaged for the respective departments, viz. Lectures at the Surry Institution.

Chemistry and Mineralogy, Mr. ACCUM.

Music, Mr. S. WESLEY.

Experimental Philosophy, Mr. JACKSON: and

Physiology (with Experiments), Dr. DAVIS.

To Correspondents.

I am not acquainted with any work on the subject after which E. H. inquires.

The papers of Mr. Barlow and Mr. Lyall will be inserted in our next number.

ERRATA.

P. 167, l. 4 from bot. for Pl. V, read Pl. VI.

168, l. 4 for complete read comp'lex.

METEOROLOGICAL JOURNAL,

For OCTOBER, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker,
in the STRAND, LONDON.

SEPT. Day of	THERMOMETER.				BAROME- TER, 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.		Day.	Night.
27	54	48	58	40	29.50	Rain	Rain
28	46	46	53	38	29.82	Ditto	Ditto
29	43	47	53	42	29.96	Fair	Fair
30	48	56	50	45	29.93	Rain	Ditto
OCT.							
1	51	52	56	49	30.14	Fair	Cloudy
2	56	58	60	55	30.28	Rain	Rain
3	56	53	61	55	30.32	Cloudy*	Cloudy
4	57	54	62	47	30.21	Rain	Fair
5	54	53	60	51	30.10	Ditto	Ditto
6	56	53	60	50	30.09	Ditto	Ditto
7	55	51	59	43	30.08	Ditto	Ditto
8	50	47	55	42	30.10	Ditto	Ditto
9	47	45	51	40	30.10	Ditto	Cloudy
10	45	45	49	41	30.04	Ditto	Ditto
11	47	45	50	41	30.02	Ditto	Fair
12	44	40	48	36	30.10	Ditto	Ditto
13	40	39	46	37	30.19	Ditto	Ditto
14	39	44	49	35	30.26	Ditto	Ditto
15	37	44	48	40	30.32	Ditto †	Cloudy
16	47	53	55	41	30.14	Cloudy	Ditto
17	54	55	58	52	30.11	Ditto	Fair
18	54	56	58	54	30.09	Rain	Cloudy
19	55	57	59	52	30.17	Ditto	Rain
20	53	53	55	51	30.18	Cloudy	Cloudy
21	52	53	55	50	30.15	Ditto	Ditto
22	52	52	55	50	30.10	Ditto	Ditto
23	53	53	56	48	30.00	Ditto	Fair
24	51	54	58	49	29.91	Rain	Ditto
25	51	53	55	50	30.17	Ditto	Ditto
26	53	56	61	48	30.28	Ditto	Ditto

* Day gloomy and close.

† Heavy fog.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

DECEMBER, 1809.

ARTICLE I.

*On Vegetable Astringents. By JOHN BOSTOCK, M. D.
Communicated by the Author.*

(*Continued from page 222.*)

AMONG the constituents of galls we always find mucilage enumerated, and Mr. Davy gives a process for obtaining it in a separate state, but I confess, that I am not altogether satisfied with the force of the arguments, by which its existence is thought to be proved. Mr. Deyeux, who I believe first distinctly mentioned the existence of mucilage in galls, founded his opinion upon an erroneous supposition, that no substance except mucus is capable of producing mould. The moulding, as has been shown above, evidently depends upon the other constituents of the infusion*. With respect to the tests for mucilages, the only one which can be considered as applying generally to them,

Mucilage, not the only substance that grows mouldy. Tests not applicable here.

* As a farther proof of this position I may remark, that I have observed the process of moulding in Mr. Hatchett's artificial tan.

and which acts upon them when not in a concentrated state, is the acetate of lead; but this unfortunately cannot be applied in the present instance, because it is equally affected by tan and the gallic acid. The other tests which I found in my former experiments on this subject* to act upon particular varieties of mucilage, such as the nitrate of mercury, the oxisulphate of iron, the nitromuriate of gold, and silicated potash†, were each of them limited to those varieties, and can therefore be of no use in determining the general question, beside that some of them act upon the other constituents of galls. There is, however, one property, in which all mucilages seem to agree, i. e. their insolubility in alcohol; and it is upon this property, that Mr. Davy has founded his operation for obtaining the mucus of galls in a separate state.

All mucilages insoluble in alcohol.

There appears to be no mucilage in galls.

I endeavoured to imitate his process, but without success. A strong infusion of galls had its tan separated by jelly, the residual fluid was evaporated, and its solid contents were boiled in alcohol, in order to remove from them any extract or gallic acid. What was left was digested in warm water; a very small quantity of it seemed to be dissolved, and the fluid assumed a light green hue. The acetate of lead threw down a slight precipitate, and left the fluid colourless; it was tinged by the oxisulphate of iron; tartarised antimony and oxalic acid had no effect upon it; it was neither acid nor alkaline; being slowly evaporated, a small gray residuum was left, which did not resemble mucilage in any of its physical properties. We come to the same conclusion respecting the existence of mucus in galls by digesting a quantity of the powder in successive quantities

* Nicholson's Journal, XVIII, 28.

† I am induced to consider the precipitate which is produced by the addition of silicated potash to gum arabic, a fact which was first noticed by Dr. Thomson (a), as depending, not upon the immediate action of silix upon gum, but upon the lime which enters pretty largely into its composition, and which causes oxalic acid to throw down a copious precipitate from it. When silicated potash is added to the different vegetable infusions, the same effects seem to ensue as from the employment of the alkali without the silix.

(a) Chemistry, V, 40.

of

of alcohol or ether; in both these cases, after the action of these fluids has been carried to its fullest extent, a residuum is left, upon which water has no action, yet mucilage is insoluble both in alcohol and in ether. I feel it necessary to apologize for differing from Mr. Davy on this point of fact, but I may say in my excuse, that he relates the process for obtaining the mucus of galls rather as one calculated to answer the end in view, than as what he had really put in practice. That portion of the galls, which in his analysis he attributed to mucus, I should refer principally to the imperfect compound of tan and jelly, which I have described above.

I shall now make a few observations on the chemical properties of catechu; but it is necessary to premise, that the varieties in this substance are even greater than those in the gall nut. That which I employed was considered by a friend, on whose judgment I could rely, as a good specimen of the kind which is most esteemed by the apothecaries; yet from my experiments with it, it seemed to differ from that employed by Mr. Davy. Cold water being digested upon it for two days took up $\frac{1}{10}$ of its weight; the solution was transparent, and of a fine reddish brown colour; the portion which remained undissolved seemed like a mixture of white and red particles, in which the white considerably predominated, but when it was dried its colour became as deep as that of the catechu in its recent state. The solution slightly reddened litmus; it was rendered turbid by the oxalate of ammonia, and a small quantity of a dense precipitate subsided from it. It was also liable to the operation of moulding, although not so readily as the infusion of galls. When catechu is treated with hot water, it is partly dissolved and partly suspended. An opake infusion is formed, which contains about $\frac{1}{10}$ of its weight of solid matter. The warm infusion still continues quite opake after being passed through a paper filter, while the filter gains a great addition of weight, and is stiffened as if it had been soaked in some kind of mucilaginous matter. By standing for some days, a part of the contents is deposited, and the warm infusion becomes transparent. If the clear solution be evaporated, the residue is not capable of being

Catechu very variable in its qualities.

Treated with cold water,

and with hot

The transparent solutions slowly deposit part of their contents,

and grow mouldy.

Requires successive infusions.

Treated with alcohol.

completely redissolved, and the second infusion is rather lighter coloured than the former one. The transparent solutions of catechu, whether formed by warm or by cold water, slowly deposit a part of their contents, in the form of the whitish residuum mentioned above, while at the same time a kind of efflorescence creeps up along the sides of the glass to some distance above the surface of the fluid. This deposition proceeds the more rapidly, the stronger is the infusion; but there does not appear to be any absolute limit to its continuance. In one instance I found, that a saturated solution of catechu, after standing two months, had lost rather more than half of its solid contents, but a part of it had been expended in forming a stratum of mould. The substance that has been deposited is less soluble in water than the recent catechu, but it dissolves readily by an increase of temperature; it forms a solution of a lighter colour, and it has less disposition to separate from the fluid. Although catechu is so readily soluble in water, yet, as is the case with galls, it requires a number of successive infusions to separate the soluble part from the small insoluble residue. Ten grains of catechu were infused in 50 times their weight of water for 24 hours; the fluid was then drawn off, and the same quantity of water was poured upon the residue. After 9 of these successive infusions, the effect of the oximuriate of tin was no longer visible, that of jelly was barely so, but the oxisulphate of iron continued to tinge the fluid until the 15th infusion, and at this period the acetate of lead produced a very slight cloud. The insoluble residue left was not more than $\frac{1}{10}$ of the weight of the catechu employed; it seemed to be a heterogeneous mass, consisting probably of accidental impurities, and it may be expected therefore to vary in quantity. Mr. Davy found no less than $\frac{1}{4}$ of the catechu upon which he operated to consist of insoluble matter*.

Alcohol, at the temperature of the atmosphere, slowly dissolves catechu. By boiling the effect is much promoted, and the alcohol takes up about $\frac{1}{20}$ of its weight, which remains permanently dissolved, but the quantity varies very

* Phil. Trans. 1803, p. 259.

much

much in different specimens. About $\frac{1}{2}$ of the catechu seems insoluble in this menstruum. This part was readily taken up by water, except a small dark-coloured residuum; the solution produced only a slight effect upon jelly and the oximuriate of tin, but by the oxisulphate of iron the whole became as it were coagulated, and was converted into a gray mass. The acetate of lead also threw down a very copious precipitate from the fluid. These properties denote a considerable analogy between this part of the catechu, and the mucilaginous bodies, an analogy which is farther strengthened by a degree of viscosity, which may be observed in its solutions. The substance obtained by evaporating the spirituous solutions of catechu is of a deep red colour, soluble in water but less so than the whole catechu; the solution is copiously precipitated by jelly, by the oximuriate of tin, and the oxisulphate of iron. It moulded by exposure to the atmosphere, I think, rather more readily than the entire catechu.

The infusion of catechu is very copiously precipitated by jelly, but a part of the precipitate generally remains suspended in the fluid. The oximuriate of tin also acts powerfully upon catechu, but it is not much affected by tartarized antimony, it is rendered opaque, and the brown colour is changed to red, but scarcely any precipitate is formed. The acetate of lead exercises the same instantaneous action on catechu as on galls; it immediately unites with all the constituents of the infusion, and leaves the fluid perfectly transparent and colourless. The nitromuriate of gold throws down a very copious precipitate of a blackish purple colour, and the nitromuriate of platina an equally copious one of a deep reddish brown. The precipitate produced by the oxisulphate of iron is of a deep olive green, and readily subsides from the fluid. This precipitate must, I apprehend, be considered as an obvious indication of a small quantity of gallic acid; and may therefore be regarded as a proof of the variety, which exists in different species of this substance, since that which Mr. Davy employed was without this constituent*. I have always found the infu-

Action of reagents on the infusion.

* Philos. Trans. 1803, p. 269.

sions which I formed to be slightly reddened by litmus. After the infusions of catechu have undergone the operation of moulding, they are much less affected both by jelly and by the oximuriate of tin, but I never carried the process so far as to observe whether they could be entirely deprived of the capacity of being acted on by these reagents.

Unsuccessful
attempts to se-
parate the tan
and extract of
catechu.

According to Mr. Davy's observations the separation of the tan and extract of catechu may be accomplished with a considerable degree of accuracy, and he points out three different ways in which this may be effected. Tan, he remarks, is more soluble in water than extract, if therefore catechu be subjected for a short time to a small quantity of water, the tan alone will be dissolved, and the residue will contain a greater proportion of extract. I infused a portion of catechu for a few minutes in about ten times its weight of water, by which a part only was dissolved. The residue was afterward dissolved by the addition of more water, and when each of the infusions was become clear, by depositing a part of their contents, they were both of them submitted to the action of jelly and the oximuriate of tin; the first infusion was stronger, but I could not observe the least difference in the proportional effects of the two reagents. Mr. Davy's 2d method of separating tan from extract is founded upon the principle, that extract is more soluble in warm than in cold water, and therefore if a saturated warm infusion be formed, when it cools the tan will remain dissolved, while the greatest part of the extract will be deposited. I put this process into execution, but upon applying the two reagents they both seemed to act in an equal degree, differing only in their effects in consequence of the matter which was deposited being rather less soluble than the entire catechu. The 3d method of separating the tan from the extract is by forming a number of successive infusions, when it is said, that the tan will become first exhausted, and the extract be left in a state of almost perfect purity. I have already related the result of this operation, which was not at all conformable to the above statement. These circumstances I regard as amounting to a positive proof of an essential difference between the substances, which were employed by Mr. Davy and myself.

In

In my former paper I mentioned, that I had performed some experiments on the extract of rhatany; which led me to conclude, that it consisted principally of tan. It readily dissolves in water, and the solution is much promoted by an increase of temperature; as the water cools, a part of the rhatany separates, leaving about $\frac{1}{10}$ of the weight of the fluid in permanent solution. The fluid very slightly reddens litmus, and after some time shows a tendency to mould. The part that is deposited from the solution by cooling does not appear to be different from what is retained by the water, except that it contains a small insoluble residuum, which I am disposed to regard as an accidental impurity, and from which it requires a number of successive infusions entirely to separate the soluble part. That part which subsides from the warm infusion is also less soluble than the entire extract; but this I attribute rather to the effect of the operation, than to any original difference in its nature. Alcohol takes up about $\frac{1}{10}$ of its weight of the extract, the solution is promoted by heat, it requires several successive applications to remove all the soluble matter, and a portion is left, upon which the alcohol has no longer any effect. This part is readily dissolved by water, and forms a solution, which is of a bright red colour, which was rendered slightly turbid by jelly and the oximuriate of tin, but was very copiously precipitated by nitromuriate of gold and the acetate of lead, the former producing a reddish brown, and the latter a delicate pink precipitate. The results are very similar to what has been related above respecting the action of alcohol upon catechu, and indicates the presence of a substance, which in its chemical characters bears an analogy to mucus; at the same time it must be remarked, that the solutions of rhatany are free from any degree of viscosity.

Rhatany treated with water assisted by heat.

and with alcohol.

Part insoluble in alcohol.

Rhatany acts very powerfully upon jelly, forming with it a light red precipitate, which generally separates from the fluid. It appears, that the most perfect compound is produced by about equal parts of prepared isinglass and extract, but the substances do not unite in the same definitive proportion, the nature of the compound being much influenced

Action of reagents on it.

fluenced by the relative quantities in which its ingredients are presented to each other. When there is an excess either of jelly or of tan, the precipitate subsides more slowly, and is of a softer texture. Beside jelly, rhatany is precipitated by oximuriate of tin, oxisulphate of iron, acetate of lead, tartarized antimony, nitromuriate of gold and of platina, alum, lime, and sulphuric acid. The oximuriate of tin throws down a dense precipitate, but only in moderate quantity: much less than that produced by jelly; the oxisulphate of iron produces a black precipitate, which speedily subsides to the bottom of the vessel; acetate of lead instantly combines with all the contents of the solution, throws them down in the form of a pink mass, and leaves the fluid transparent and colourless; antimony throws down a small quantity of a reddish powder; the nitromuriate of gold produces a very copious dark purple precipitate, and the nitromuriate of platina an equally copious one of a reddish brown colour. Alum renders the solution turbid, changes its colour to a dirty brown, and throws down a small quantity of precipitate; lime water heightens the colour, and produces a red precipitate; and sulphuric acid produces a copious precipitate of a light red colour. Carbonate of potash converts the colour of the solution to a deep blood red, but produces no farther effect. After the infusion of rhatany has had jelly added until no further precipitation is produced, the oximuriate of tin renders it slightly turbid, but can scarcely be said to form a precipitate; if however, the experiment be reversed, i. e. if jelly be added to the infusion after the action of the oximuriate of tin, a copious precipitate is thrown down.

Is tan always identical?

It has been a much agitated point, whether tan be in all cases uniform in its properties, or whether there may not be substances possessed of the leading characteristics of tan, particularly its property of precipitating jelly, which yet, in some respects, may differ from each other. This latter opinion has been adopted by Proust; while Mr. Davy, on the contrary, appears inclined to attribute any diversity of operation on the different reagents, not to any difference in the tan itself, but to the peculiar substances with which it may

be united. I confess that I am disposed to adopt the doctrine of Proust: for although Mr. Davy's remark be correct, that in all vegetables, in which tan has been discovered, it exists in a state of combination with other principles, and that its action must necessarily be modified by these combinations; yet I conceive, that, as far as we are able to judge, the nature of the combinations will not account for the difference of the effects. The extract of rhatany is copiously precipitated by jelly, and considerably so by the oximuriate of tin; but as this reagent produces scarcely any effect after the addition of jelly, we must conclude, according to the generally received opinion, that the effect of both these substances depends upon the tan which it contains, so that we are led to regard it as consisting of tan, combined with a little mucilage and a minute portion of gallic acid. Yet we find, that tartarized antimony and the carbonate of potash, which act so powerfully upon the tan of the gall-nut, scarcely produce any precipitate with the tan of rhatany. We must therefore conclude, either that the action of the oxide of antimony and the carbonate of potash depends upon the presence of some extraneous body, or that there may be a substance, which forms an insoluble compound with jelly, and which, on this account, is entitled to the appellation of tan; but which may be so modified, as in some states to unite with the above reagents, and at other times to have no effect upon them. Considering the magnitude of the effect produced, compared to the supposed nature of these extraneous bodies, I cannot but think the latter opinion the more probable. This, it is admitted, is no more than a presumptive argument; but I apprehend, that the same point is more firmly established by what I have observed respecting Mr. Hatchett's artificial tan. This substance we may regard as homogeneous, and therefore not liable to those objections, which apply to such experiments as are performed upon any of the vegetable infusions; and, yet I have found it to act very differently upon other reagents, at the same time that it exercised the most powerful action upon jelly. I have now before me a solution of the artificial tan, which copiously precipitates jelly, the oximuriate

Probably not.

Artificial tan.

muriate of tin, the oxisulphate of iron, the acetate of lead, alum, lime water, and sulphuric acid; and yet it is very slightly affected by tartarized antimony, and not in the least by the carbonate of potash. Are we then to conclude, that pure tan, such as may be supposed to exist in Mr. Hatchett's preparation, has no affinity for the oxide of antimony and the carbonate of potash; and that, when the tan of the gall-nut is precipitated by these reagents, it depends upon a primary action, which they exert upon some other constituent? or that there may be substances, which have some specific differences, although, from their leading properties, they may be all of them strictly entitled to the generic name of tan? The coincidence between Mr. Hatchett's tan and rhatany, so far as the reagents are concerned, might seem to favour the former opinion; yet the latter supposition implies nothing that is improbable, and is agreeable to the analogy, which prevails in other vegetable productions.

With respect to any general conclusions, that I may draw from my experiments on these different vegetable astringents, I feel so well aware of the difficulty of obtaining unexceptionable results, and the uncertainty of the inferences that ought to be deduced from them, that I shall not venture to consider the positions which I have advanced as ascertained matters of fact, but rather as subjects for future investigation. All that I can expect from this paper is, that it will serve as an addition to the store of information which is daily accumulating, and which may assist at some future period in laying the foundation of a more matured theory, than any which could be constructed at present,

Liverpool, October 10, 1809.

II.

Memoir on the triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. LE COMTE DE BOURNON, F. R. and L. S.

(Continued from page 237.)

Determination of the primitive crystal of endellion.

FROM the direction of the laminæ of crystallization of the endellion, with which some accidental fractures had made me perfectly acquainted, as well as from the general aspect of the crystals of this substance, I could not doubt, that the form of its primitive crystal was a rectangular tetraedral prism; and the similitude of the retrogradation of the crystalline laminæ along the edges of the terminal faces, indicated by the equality of the inclinations of the faces belonging to them, made me presume, that these faces must be squares. Accordingly it appeared to me, that the primitive crystal could only be a cube, or a rectangular tetraedral prism with square bases, the altitude of which would be greater or less than the side of the terminal faces.

Now remarking, that, with very few exceptions, in all substances, that have a perfectly symmetrical solid for their primitive crystal, as the cube, rhomboid, octaedron, regular tetraedron, &c., the secondary forms, produced by the various retrogradations to which they may be subjected, retain the same symmetry; I observe, that, in the crystals belonging to the secondary faces of the endellion, there is no symmetry between the planes that supply the place of the edges of the terminal faces, and those of the longitudinal edges of the prism, either with respect to the number of these planes, or to their inclination; whence I am naturally led to infer, that the rectangular tetraedral prism, the primitive crystal of endellion, is not a cube. Of this I was fully convinced, when I presented my first paper on this substance to the Royal Society; and it was this, that then prevented me from giving the dimensions of its rectangular tetraedral

Primitive crystal ascertained.

Where a primitive crystal is symmetrical, the secondary crystals are so likewise.

tetraedral prism, as I could not sufficiently depend on the angles of incidence, which the secondary faces, I had then seen, enabled me to take; accordingly I contented myself with giving a very near approximation to the measure of these angles, without subjecting them to the scrutiny of calculation*.

Mode in which
the primitive
crystal was as-
certained.

Circumstances having since enabled me to acquire perfect certainty with respect to the measures of these angles, it now remains for me to determine the dimensions of the tetraedral prism. To effect this, after recognizing four different retrogradations along the edges of the terminal faces of the prism, as well as four others at the angles of the same faces, I observe, that the dimensions of the crystal may be determined either by the first of these retrogradations, or by the second: I observe too, that each will serve reciprocally as a support or confirmation of the other.

Directing my attention in the first place to the four retrogradations, that take place along the edges of the terminal faces, I begin by taking as accurately as possible the angle formed by the planes arising from each of these retrogradations with the terminal faces of the prism, and find one angle about 130° , another about 135° , a third about 149° , and the fourth between 171° and 172° . I then draw a horizontal line, A B C, fig. 28, representing one of the edges of the terminal face of a crystal perfectly resembling the primitive one, but composed of a certain number of crystalline molecules united, and the equal divisions of

* *Additional note.* It was not from omission therefore, but for valid reasons, that I did not give the cube as the primitive crystal of this substance. Mr. Smithson, to whom I am very well known, might have done me the justice to suppose, that, if the determination of this crystal, from the facts I could then observe, had been as simple as his calculation indicates, my eyes had too much experience in crystallography, for it to have escaped me. If however he had entertained any doubts on this point, he was sufficiently acquainted with me, to have communicated them to me in a less hostile manner; when I would with great pleasure have submitted to him the reasons, that had determined me to act as I did. By this science would have lost nothing, and I should have gained much, in probably not experiencing the extraordinary, and I will boldly say unmerited conduct, that has been held toward me in the name of the Royal Society, of which I am proud to call myself a member, and for which I shall always feel the highest respect.

which

which at the points A, T, B, C represent the extremities of the several edges of the component crystals. From the extremity C of this I let fall the perpendicular Ch, representing the direction of the side, or altitude, of the prism, and the length of which I leave undetermined. Through the extremity of this line, C, I draw the lines G C P, F C O, E C N, D C M, forming with it angles of 130° , 135° , 149° , and $171^\circ 30'$, which I produce indefinitely above the point C. From the point B, the extremity of the side of the first molecule, I erect the indeterminate perpendicular B G, cutting all the preceding lines. It is evident, that the lines C G, C F, C E, and C D, will indicate the direction of the planes derived from the four different retrogradations, that take place along the edges of the terminal faces; and that, if one of them be made by a single row, the part of the perpendicular B G, included between the line of the direction of the plane derived from this retrogradation and the line A C, will represent the height of the molecule of the last lamina placed on it, and consequently that of the primitive crystal.

Mode in which the primitive crystal was ascertained.

It remains now to inquire, which of these retrogradations was most probably made by a single row; and whether, after having determined this, all the others will agree with it. The angle F C B, or that of inclination between F C and A C, being 45° , or the supplement of B C O, which was by construction 135° , would indicate a height equal to the edges of the terminal faces, and consequently the cube as the primitive crystal; and the observation already made militates against the choice of this, unless the farther observations, in which we are engaged, oblige us to adopt it. The angle of inclination, G C B, of the line G C, would indicate a height greater than that of the cube; and in all the crystals of this substance the longitudinal edges of the prism being constantly shorter than those of the terminal faces, I am led rather to reject than adopt this height, which would give 28.6, the edges of the terminal faces being supposed 24. The choice then remains between the two retrogradations represented by the two lines E C and D C, the first indicating E B for the height, and the second D B; and the resolution of the two rectangular triangles

E B C

Mode in which
the primitive
crystal was as-
certained.

EBC and **DBC** will very readily give the value of these two lines with respect to the edge of the terminal faces represented by **BC**, and which we have already supposed to be 24. But as the height **DB**, which then would be about 3.58, would not agree with the inclination of the planes belonging to the other statements, the choice cannot remain doubtful, it falling necessarily on **EB**, which is of 14.4, and consequently to the edge of the terminal faces in the ratio of 14.4 to 24, or of 3 to 5. And in fact, on fixing at this the height of the primitive rectangular tetraedral prism, the determination of the other statements by calculation agrees perfectly with the inclination found by measuring the planes arising from them. Besides, the height **DB** of 3.58 would be much too small with respect to what all the crystals of this substance exhibit; while on the contrary that of 14.4 agrees with every thing found by observation in these crystals.

It remains now to examine, whether the retrogradations at the angles of the terminal faces agree with this height; and, if they should not agree with it, whether they do not point to one more natural, and more fit to be adopted. To proceed on this examination, I take with the instrument, as accurately as possible, the angle of incidence between the terminal faces and the four planes which take the places of their angles. Their measurement gives me 125° for one, between 134° and 135° for another, between 150° and 151° for the third, and about 172° for the fourth.

The terminal faces being a perfect square, fig. 30, and the side of the square being assumed 24, the diagonal **RS** is 33.94, and consequently its half is 16.97.

As every retrogradation at the angles of a polygon is made on the diagonal passing through these angles, if we suppose the primitive rectangular tetraedral prism, of which fig. 30 represents the terminal face, cut by a plane passing through the diagonal **RS** and that which is opposite to it in the lower face, all the diagonals of the molecules of the superficial lamina, on which the retrogradation is made, as well as of those superimposed on it, will be placed on the diagonal **RS**, or parallel to it. Let **QS**, therefore, fig. 29, representing this diagonal, be drawn horizontally, and
divided

divided at the points *VW* into equal parts, which shall be to those of the side *AC*, fig. 28, of the terminal faces, in the ratio of 33·94 to 24: these divisions representing the diagonals of the crystal'line molecules placed on the whole diagonal *QS*. From the point *S*, the extremity of the line *QS*, draw the lines *gSr*, *eSa*, *qSd*, and *mSf*, so as to make with this line angles of 125° , $134^\circ 30'$, $150^\circ 30'$, and 172° , and produced indefinitely above the point *S*. The lines *Sg*, *Se*, *Sq*, and *Sm*, will represent the direction of the planes produced by the four different retrogradations, that take place at the angles of the terminal faces of the primitive prism. As every retrogradation, that takes place at the angles of crystals by diagonals, is equivalent in the effect it produces to a retrogradation that takes place simply by semidiagonals; to find the height, which, that of the four that takes place only by a single row would give, in order to see whether it would accord better with nature than that of 3 to 5 given by the observations that have been made on the retrogradation along the edges of the terminal faces; from the point *R*, half of the diagonal *WS*, erect the perpendicular *Rg*, cutting the four lines *Sg*, *Se*, *Sq*, and *Sm*, representing the directions of the substituted planes. Inquiring now whether any of these planes may be produced by the simple retrogradation of a single row, I perceive immediately, that for the same reason as was given respecting the retrogradation along the edges of the terminal faces, those planes must be excluded, the direction of which is represented by the lines *eS* and *gS*. There remain then those denoted by the lines *qS* and *mS*. For the same reason likewise as was given before, that which answers to the direction *mS* cannot be adopted; consequently our choice is confined to that in the direction *qS*. The resolution of the rectangular triangle *qRS* would give 9·6 for the height of the molecule, the side of the terminal faces being still supposed 24: so that this height would be to the side in the ratio of 9·6 to 24, or of 2 to 5. Observing then, that the result of the calculation made with the ratio of 3 to 5 agrees better with what the inclination of the secondary faces of the endellion exhibits in nature, than that made with the latter ratio: remarking too, that the

Mode in which the primitive crystal was ascertained.

same

same ratio of 3 to 5 accords better with the customary dimensions of the crystals of this substance, almost all these crystals exhibiting this proportion between their height and breadth, while I have not yet found one in the proportion of 2 to 5: in determining the ratio of the height, or side of the primitive prism to the side of its terminal faces, I fix on the proportion of 3 to 5, to which I was before guided by the observation of the retrogradations that take place along the edges of the terminal faces. In consequence I conclude, that the primitive crystal of endellion is a rectangular tetraedral prism, the height of which is to the edges of the terminal faces in the ratio of 3 to 5.

The determination of a primitive crystal, with sufficient data, a simple process.

I have not hesitated to give with considerable minuteness the method I pursued in determining the primitive crystal of this substance; in the first place because it renders the Royal Society better acquainted with the grounds on which it is established, and shows, that this determination is by no means the result of an opinion adopted at first sight, or of a slight observation of a single crystal merely: and secondly, because these details show how simple and easy such determinations are, when nature supplies us with sufficient data, and at the same time how far the calculations they require are from being complicated*. The same may be said of the calculations for determining the planes produced in crystals by retrogradations of the crystalline laminae: they never require any thing more than the resolution of a triangle, for which there are always sufficient data. I think I may affirm, that, by means of the method given in my Treatise on Mineralogy; and the use of the protractor with a movable radius, which I have likewise made known, and which greatly abridges the trials we are sometimes obliged to make, for determining from the angles of incidence of the secondary planes of the crystals the nature of the retrogradations calculated to give rise to them; there exists no

* *Additional note.* This method having never yet been given in any work on mineralogy in so simple and easy a manner, and besides the Philosophical Transactions hitherto containing little on the subject of crystallography were farther inducements for me to enter into these particulars. To me it appears, that they cannot but render this paper more interesting.

science,

science, the application of which is more easy, than crystallography.

From the angle of incidence of 135° between the terminal faces and one of the planes that are substituted for the edges of those faces in the rectangular tetraedral prism of the endellion; from that nearly of the same number of degrees, which one of the planes substituted for their angles makes with the same faces; and lastly, from another of 135° , which one of the planes substituted for the longitudinal edges makes with the sides of the prism; we may be very easily led, if we confine our observations to these facts, to consider the primitive crystal as a cube. These angles of incidence however, either exact, or so near it that the instrument cannot detect the difference, may be produced by retrogradations of a number of rectangular tetraedral prisms by no means of a cubical figure. I have assigned the reasons, which have appeared to me to militate against our acceding to this first attempt. I am persuaded we much too readily yield to an inclination to consider, as cubical the primitive crystals of substances, the secondary forms of which indicate a rectangular tetraedral prism. There are already a sufficient number of substances, that really have the cube for their primitive crystal, though their integrant molecules are of a different form, without our enlarging it unnecessarily. By doing thus we afford an additional handle to those persons, who, seeing in every primitive crystal nothing but the form of the integrant molecules, from which however it is frequently very remote, make of the numerous repetitions of this form in several mineral substances, that are totally different, the grounds of a very unfounded objection to crystallography.

Circumstances
that might lead
into error.

There is a fact relating to this substance worthy of remark, which is the equality of the number of retrogradations made both along the edges and at the angles of the terminal faces, and the great analogy between the planes owing to them.

TABLE of the MODIFICATIONS of the PRIMITIVE CRYSTAL of ENDELLION.

Primitive crystal. | A rectangular tetraedral prism with square bases, in which the side or edge of the prism is to the side of the base in the ratio of 3 to 5.

Prismatic Modifications.

Retrogradations made at the longitudinal edges of the prism.

No. of the modifications.	Form of the crystal.	Angles formed by the meeting of the planes of the crystals.				Nature of the retrogradations.
		Angles that the new planes form with each other when the modification is complete.	Least obtuse.	With the primitive planes.	With the planes of the prism of the 1st modification.	
1st,	A rectangular tetraedral prism, an inversion of the primitive.	90°	90°	135°		By 1 row along the longitudinal edges of the prism.
2d,	In the complete state, a right octaedral prism.	4 angles of 138° 52'	4 angles of 131° 8'	1 angle of 156° 26'	1 angle of 155° 54'	By 8 rows in breadth, and 3 laminae in height, along the longitudinal edges of the prism.
3d,	In the complete state, a right octaedral prism differing from the former.	4 angles of 148° 6'	4 angles of 121° 54'	1 angle of 164° 3'	1 angle of 150° 57'	By 7 rows in breadth, and 2 laminae in height, along the longitudinal edges of the prism.

Pyramidal Modifications.

Retrogradations made at the edges of the terminal faces.

No. of the modification.	Form of the crystal.	Angles that the new planes form with those of the primitive crystal		Angles that the new planes form with those of the other modifications made also along the edges of the terminal faces.			Nature of the retrogradations.
		With the terminal faces.	With the longitudinal planes.	With the planes of the 4th modification.	With the planes of the 5th modification.	With the planes of the 6th modification.	
4th,	In these 4 modifications the primitive crystal has the edges of its terminal faces supplied each by a single plane differently inclined in each modification.	129° 48'	140° 12'				By 1 row in breadth, and 2 laminae in height, along the edges of the terminal faces.
5th,		135°	135°	174° 48'			By 3 rows in breadth, and 5 laminae in height, along the edges of the terminal faces.
6th,		149° 2'	120° 58'	160° 46'	165° 58'		By 1 row in breadth along the edges of the terminal faces.
7th,		171° 28'	98° 32'	138° 20'	143° 32'	157° 34'	By 4 rows in breadth along the edges of the terminal faces.

Retrogradations made at the angles of the terminal faces.

No. of the modification.	Form of the crystal.	Angles that the new planes form where they meet those of the primitive crystal.			Angles that the new planes form with those of the other modifications made also at the angles of the terminal faces.			Nature of the retrogradations.
		With the terminal faces.	With the longitudinal faces.		With the planes of the 8th modification.	With the planes of the 9th modification.	With the planes of the 10th modification.	
8th,	In these 4 modifications the primitive crystal has the angles of its terminal faces supplied each by a single plane, differently inclined in each modification.	125° 16'	144° 4'					By 3 rows in breadth, and 5 laminae in height, at the angles of the terminal faces.
9th,		134° 39'	135° 31'		170° 47'			By 5 rows in breadth, and 6 laminae in height, at the angles of the terminal faces.
10th,		150° 30'	119° 30'		154° 46'	163° 56'		By 3 rows in breadth, and 2 laminae in height, at the angles of the terminal faces.
11th,		171° 57'	98° 3'		133° 19'	142° 32'	158° 33'	By 6 rows in breadth at the angles of the terminal faces.

Crystallization of Cundellion. by the Comte de Bournon.

Fig. 1.

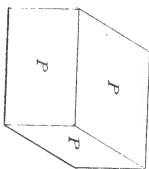


Fig. 2.

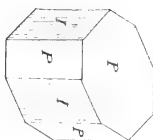


Fig. 3.

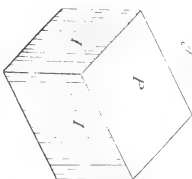


Fig. 4.

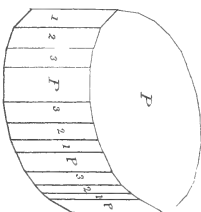


Fig. 5.

Fig. 6.

Fig. 15.

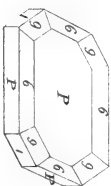


Fig. 16.

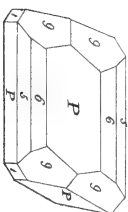


Fig. 17.

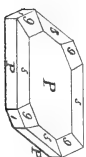
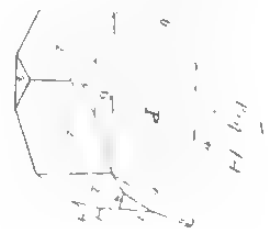
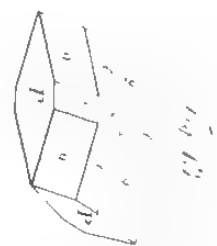
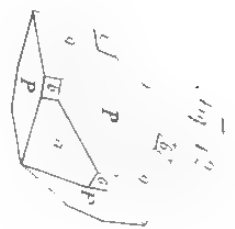
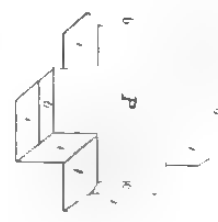
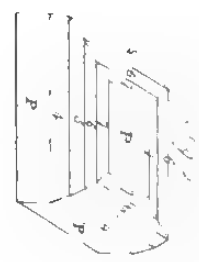


Fig. 18.



Crystallization of Condensation by the Count de Breuer

Fig. 1



Crystallization of Sndellum. by the Count de Bournon.

Fig 19

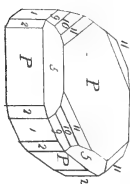


Fig 20

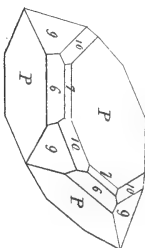


Fig 21

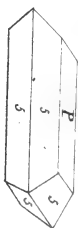


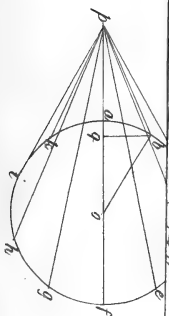
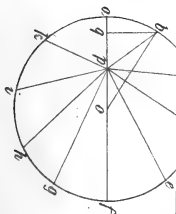
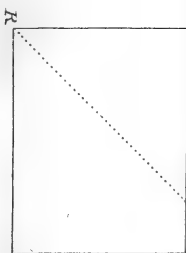
Fig. 22.



Fig. 23.



Fig. 24.



Enrollment of Endellion by the Count de Barenen

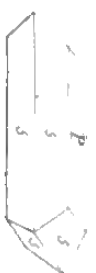


Fig 28

Fig 29

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Fig 30

Fig 31

Fig 32

Fig 33

Fig 34

III

Of the Irritability of Vegetables. By Mr. ROBERT LYALL, Surgeon. Read at the Literary and Philosophical Society at Manchester, Oct. the 6th, 1809. Communicated by the Author.

THE irritability of some plants has attracted much attention from physiologists. Some of the most eminent men have without hesitation allowed, that vegetables possess the *faculty* of irritability; while others most strenuously have endeavoured to disprove, that any such principle exists in the vegetable kingdom. As soon as the *mimosa sensitiva* was discovered, without doubt the motion of its leaves when irritated by a stimulus were observed, but at what time the cause of this motion got the title of irritability is perhaps not so certain. Haller was probably among the earliest, who ascribed the motions of some plants to an irritable principle. After speaking of the comparative irritability of the heart, muscles, and intestines, with that of the ligaments, tendons, &c., he proceeds thus:

Irritability of vegetables questioned.

“ That this irritability exists abundantly throughout the animal fibres, appears evidently from the example, which we have in the polypi and other insects, which have neither brain nor nerves, but are notwithstanding extremely impatient of all stimulus; and lastly we may take into consideration the analogy of some plants, the flowers and leaves of which either expand or contract by various degrees of heat and cold, and some even with a degree of celerity not inferior to that of animals. This force is a different and new principle from all other properties of bodies hitherto known. We cannot account for it either by gravity, attraction, or elasticity but by something which exists in the soft fibres, and which vanishes by drying*.” Haller afterward in his *Elements of Physiology* remarks, “ It is evident, that there abounds, not only in the animal but equally in the vegetable kingdom, a contrac-

Haller's account of it.

* *Prinæ Lineæ Phys.* p. 152. Ed. 1758.

“ file

“ the power, by which the elementary fibres are drawn toward each other*.”

Many consider it as admitted.

Many authors, as Gmelin, Smith, Darwin, &c., have a long time since used the word irritability, when speaking of the motions of the parts of vegetables; and in the present time it is nearly as common to talk of the irritability of the vegetable as of that of the animal kingdom. When I began this paper, it was my intention, to have taken a compendious view of all that had been said on vegetable irritability; but the subject having now become so extensive, I found time would not permit me to make those experiments, which would have been requisite either to have proved that the vegetable kingdom in general was endowed with irritability, or to have disproved it. I have therefore contented myself at present with detailing some experiments made on particular plants: and even from these I hope to prove, that, if we do not admit that vegetables possess irritability, at least that they are possessed of something which is adequate to the muscular power in the animal body; and I am convinced, that without admitting this, many beautiful phenomena must perhaps for ever baffle the attempts of physiologists to explain them.

Sennebier unwilling to allow it.

One of the most celebrated vegetable physiologists, Sennebier has already treated extensively on this subject. He related almost every thing then known concerning the motions of vegetables, but has always been unwilling to allow, that an irritable principle had any share in these motions, and has tried to explain every phenomenon (which was before thought a proof of irritability) on mechanical principles. In the following pages I intend to quote some of his opinions, and endeavour to overturn them.

His definition of it.

As confusion might arise from the word irritability not being well defined, it becomes first necessary, to have Sennebier's opinion on this point, and then to fix what we understand by it at present. “ Sennebier says: “ Irritability “ is that property, which forces a body to contract itself “ when it is acted upon in a manner proper to produce this “ effect; animals manifest this contraction in their muscles in consequence of burning, pricking, or the contact

* Elem. Phys. vol. 4, p. 440.

“ of some acrid fluid, either corrosive or spirituous; then
 “ the irritated body regains its former state, and the convul-
 “ sions are often repeated, although the impression, which
 “ is the cause of them, is not renewed. We can sometimes
 “ recall these motions when they are finished, by the same
 “ means which at first produced them. This irritability
 “ shows itself by the action of a stimulant, which may be
 “ of a very different nature, but always appropriated to the
 “ muscle which it ought to move*.” In another place he
 says: “ We have pushed the analogy between animals and
 “ plants too far. If we understand by irritability the power
 “ of being affected by foreign bodies, it will be found in
 “ almost all organized bodies; if we understand the volun-
 “ tary command of a muscular force, the analogy subsists
 “ no longer†.”

I am willing to adhere to the definition, which Senne- Volition not
 bier first states, but cannot agree with him in admitting the necessarily
 latter; for I do not consider that volition in every instance connected with
 is connected with irritability. We know well, that the sen- irritability.
 sible iris often contracts and dilates without our knowledge
 of it. Here then volition, or a voluntary command of a
 muscular power, is out of the question; yet none will deny,
 but that the iris is one of the most irritable parts of the ani-
 mal body. We also sometimes observe, that the motions of
 the iris continue, when all voluntary power is at the mo-
 ment suspended, as in certain cases of concussion of the
 brain, &c. We know also, that the most important func-
 tions in the animal system are carried on quite independent
 of the will. The heart for instance is continually acting,
 yet we are unconscious of it. There are many other mus-
 cles &c., which also act independent of the will, as the diaph-
 ragm, the muscles of the intestines, and even at times the
 sphincter muscles, &c. Hence some of them have been de-
 nominated involuntary muscles. We have now seen then,
 that motions go on in the living animal system without the
 concurrence of the will, and yet that the parts are highly ir-
 ritable. Bearing this idea in mind, cannot we conceive, that

* Sennebier's *Physiologie Vegetale*, tome V, p. 87.

† *Physiol. Vegetal.* tome V, page 120.,

the motions of plants may still be owing to irritability, although there is no mind to regulate them? I do not mean here to say, that plants have not a voluntary power; but merely admit that they have not in the present question. Willdenow, when treating of the powers which vegetables possess, speaks thus on the subject. "Irritability, when different stimuli produce a change in the parts of a body, which without it would not have taken place*."

The author's definition of the term.

The following definition seems to me as comprehensive and accurate as any, and shows the meaning in which the word irritability is used in the following pages. By irritability then I understand that property inherent in some bodies, (or rather parts of bodies) by which, when a stimulus is applied, they are enabled to contract.

Plants experimented on by the author.

Having thus fixed the definition, let us proceed to the immediate subject of the paper. The plants on which I made my experiments were the *drosera rotundifolia* and *longifolia*, the *euphorbia helioscopia*, and the *dionœa muscipula*. The species of *drosera* come first under consideration; but before speaking of the moving power of their leaves, I think it necessary to give a minute description of them: and 1st of the *rotundifolia*.

Leaves of roundleaved sundew described.

The leaves of this plant, when properly unfolded, lie round the stem in a stellated manner. The footstalks of the leaves vary in length from half an inch to one inch and half. On the upper side they are a little roundish, and have at the same time a two edged appearance. The under surface is quite flat, and bounded by the two edges just mentioned. They are of a reddish colour, and are covered by a great number of long white hairs, spreading in different directions. They may be bent considerably without breaking, and, when the resisting force is removed, resume their former situation. At the end of the footstalks we find the leaves generally of an orbicular shape, hence the specific name of the plant. The under surface of the leaf is in the same plane with the under surface of the footstalk, (indeed it is difficult to say where the one ends and the other begins) has a somewhat membranaceous appearance, and is in general of a greenish (though sometimes purplish) colour. The

* Principl. Botany & Veg. Physiol. Willdenow (Translat. p. 219.)

upper

upper surface is covered with hairs, but when deprived of them appears nearly like the under. The leaf itself is very thin, and may be folded in different directions without breaking. The hairs, which cover the upper surface of the leaves, are of various lengths. Those on the margin are sometimes three eighths of an inch long, while those in the centre are not more than one line. In some well expanded leaves we may see nearly a regular gradation of the intermediate hairs between the two extremes. The marginal hairs are flattish at their base, and of the same colour with the leaf itself, (indeed they seem to be merely continuations of it). The other hairs are not so flat at their base as the marginal ones, but are also of the same colour as the leaf. The long hairs, except at their base, are of a red colour, each terminated by a little knob; while in the central hairs the knob is placed immediately on the white part of each. Every hair then is terminated by a little rounded body. In general each of these knobs is covered by a transparent and viscid fluid, which gives a fine appearance, and on account of which the plant was denominated *ros solis* or *sundew*. Each of these knobs appears to be a little gland, which secretes the viscid fluid* for a purpose soon to be mentioned.

The chief difference between the leaves of the *longifolia* Longleaved. and the *rotundifolia* is in the shape, those of the former being obovate.

* This fluid, which covers the glands of the hairs (of the leaves) of the indigenous species of *drosera*, has been differently denominated. Fluid of drosera.

Darwin talks of the pellucid drop of mucilage on every thread of the fringe, and in the same page speaks of the globules of mucus. Bot. Gard. Roth calls it the clammy juice, and I have here called it a transparent viscid fluid. This juice covers the glands when under the influence of the hottest sun; and also during the wettest weather. When the leaf is put under the electrical influence, each little globule of fluid spins out like a small tree, presenting a fine appearance. This fluid seems to possess the following properties, although I cannot vouch for the accuracy of the experiments. It is transparent, insipid, rather more consistent than the albumen ovi, extremely tenacious, insoluble in water, soluble in alcohol, in diluted sulphuric acid, and in solution of potash, is not very combustible, and is an electric or nonconductor. Quere. Presents a beautiful appearance when electrified. Its properties.

Is it a fluid sui generis? It certainly deserves the attention of the chemist,

The

Mr. Whately noticed their contraction when irritated. The first mention of the contraction of the leaves of the species of *droseræ*, at least in this kingdom, when irritated, will be found in Withering's Botany (vol. 2d p. 324) from which it would appear, that Mr. Whately discovered this curious phenomenon in August 1780. Mr. Gardom, who was with Mr. Whately at the time, gives the following account of This described. this contraction in a letter to Dr. Withering. "In August, 1780, examining the *drosera* in company with Mr. Whately, on his inspecting some of the contracted leaves, we observed a small insect or fly very closely imprisoned therein, which occasioned some astonishment, to me at least, how it happened to get into that confined situation. Afterward, on Mr. Whately's centrically pressing with a pin other leaves yet in their natural and expanded form, we observed a remarkable sudden and elastic spring of the leaves, so as to become inverted upwards, and as it were incircling the pin, which evidently showed the method by which the fly came into its embarrassing situation. This experiment was renewed repeatedly, and with the same effect, so that Mr. Whately and myself are both certain of the fact."

Roth observed it earlier. Roth published his work entitled *Beitræge zur Botanick* in 1782, from which Dr. Withering translates the following remarks. "July 1779. *Drosera rotundifolia* and longifo-

His account of it. lia. I remarked, that many leaves were folded together from the point towards the base, and that all the hairs were bent like a bow, but there was no apparent change in the leaf stalk. Upon opening these leaves, I found in each a dead insect. Hence I imagined, that this plant, which has some resemblance to the *dionœa muscipula*, might also have a similar moving power. With a pair of pliers I placed an ant upon the middle of a leaf of the *drosera rotundifolia*, but so as not to disturb the plant. The ant endeavoured to escape, but was held fast by the clammy juice at the points of the hairs, which was drawn out by its feet into fine threads; in some minutes the short hairs on the disk of the leaf began to bend, then the long hairs, and laid themselves upon the insect. After a while the leaf began to bend, and in some hours the end of the leaf was so bent inwards as to touch the base.

"The

“ The ant died in 15 minutes, which was before all the hairs
 “ had bent themselves. On repeating this experiment, I
 “ found the effects to follow sooner or later according to the
 “ state of the weather. At eleven in the morning a small
 “ fly, placed in the centre of the leaf, died sooner than the
 “ ant had done, the hairs bent themselves as before, and at
 “ five in the evening the leaf was bent together, and held
 “ the fly shut up. The same experiments being made on
 “ the *drosera longifolia*, the same effects followed, but more
 “ rapidly. I observed, that in sultry weather, and hot sun-
 “ shine, when the drops of juice upon the points of the
 “ hairs are largest, the experiment succeeds best. If the
 “ insect be a small one, sometimes only one edge of the leaf is
 “ folded up; hence it should seem necessary, that the in-
 “ sect should stir all the hairs of the leaf.”

Roth also found, that the hairs bent themselves when he touched them with the point of a needle, with a hog's bristle, &c.; but that they returned to their former position after a certain time. He remarked the same contraction when he placed a piece of wood the weight of an ant upon the leaves; but that the impression made by the point of a needle remained longest. Although Withering points out the most of these circumstances particularly with a view to excite the attention of botanists to the species of *drosera*, yet I have not met with any account of experiments made since the time he wrote; in the year 1796.

Found to con-
tract when
touched by a
needle.

For the last 5 months of the present year I have almost every day had these plants under my eye, either at home or abroad in the country. For my own part I must confess, that I have never seen that rapid contraction of the leaves of the *drosera rotunda*, which is mentioned by Mr. Gardom; but in all the experiments which I have made, I have observed that the contraction was gradual, though it seldom failed to happen, if the plant was in good condition. In some plants I have seen the contraction take place in nearly the time mentioned by Roth; but in most cases it has happened, that an hour was necessary for the complete bending of all the hairs, and that it required some hours more for the perfect shutting up of the leaves. In some plants I have seen the hairs and leaves nearly expanded even some hours

The contrac-
tion takes
place but
slowly.

hours after the stimulus was applied, and yet in the course of a few more hours both have contracted completely. The last experiments were made within doors, and probably the plants, though to appearance pretty fresh, were not in the most irritable state.

Experiment failed with Dr. Withering: probably because he did not wait long enough.

Dr. Withering mentions, that Mr. Whately's experiment failed in his hands, and from Roth's and the above observations we possibly may account for this. From what Mr. Gardom has said, he no doubt expected a sudden contraction of the leaf when irritated; but not finding this to happen, he probably concluded, that the plant was not in good condition, and, from placing implicit faith in Mr. Gardom's experiments, was not anxious to repeat them. I do not mean by what I have said, to impute to Mr. Gardom any inaccuracy in the relation of his experiments, but merely to put others on their guard, who wish to make experiments on this plant. From what I have said it is evident, that whoever has a wish to notice the motions of the leaves of the *droseræ* must not set out with the expectation of seeing a rapid motion (similar to what happens in the *mimosæ*) follow the application of a stimulus; but, to observe the ultimate effects, must watch with an attentive eye for at least, in general, 20 minutes. It is then that he will behold the bending of the hairs, which will soon be accompanied with that of the leaf.

Manner in which the contraction is accounted for

Having now considered the motions of the leaves of these plants, let us examine the manner in which they are accounted for. Broussonnet, in a memoir of the Academy of Sciences of Paris for 1784, suspects, that the disengagement of some fluids influences these motions. As Broussonnet's theory is quoted by Sennebier, I shall translate the words of the latter. After speaking of the *dionœa muscipula*, he says: "He (Broussonnet) remarked the same phenomena upon two species of the *drosera*; their leaves at first being folded upon themselves, their juices are not carried immediately towards the little hairs which cover them, but after their developement we can perceive a drop of fluid towards the extremity of each hair; the insect absorbing this fluid, empties the vessels of the leaf, which folds upon itself, and resumes its former position:

by Broussonnet.

"the

“ the quickness of this action is then proportional to the number of hairs touched by the insect *.”

This theory at first reading does not appear even to be plausible; for how is it possible, that an insect can absorb a thick tenacious fluid? No doubt however part of this fluid will be attached to the part of the insect which touches it, but this seems quite unconnected with the contraction of the leaf, as I shall immediately show. On the 30th of July I brought from the country a number of plants of the *Drosera rotundifolia*, and on inspecting them I found many of the hairs deprived of their viscid fluid, but yet both they and the leaf remained quite expanded and in good condition. This appeared to me a favourable opportunity, to ascertain either the accuracy or inaccuracy of Broussonnet's theory. Next day in the afternoon about four o'clock, when rather cloudy and the temperature moderate, I placed a small bit of sulphate of copper in the disk of one of these expanded leaves. Now if Broussonnet's theory was accurate, I conceive, no effect should have taken place; but on the contrary by six o'clock most of the hairs on one side of the leaf, even the outermost, had bent themselves completely over the morsel of sulphate of copper. I have repeated this experiment frequently, and always with the same result. It may be well also to observe, that in other experiments the sulphate of copper rested upon some of the small hairs in the disk of the leaf without touching the leaf itself, yet the bending of the hairs and leaf was complete. In some plants also, in which every hair of the leaf has been covered with a drop of viscid fluid, I cautiously placed a small bit of bread, or wood, on three or four of the central hairs without touching the other hairs, or the viscid fluid on their ends, and in the course of a few hours I found, that all the hairs had contracted around the foreign body.

We have here proof then, 1st, That the leaves do not contract when deprived of this viscid fluid; which ought to have been the case, if Broussonnet's theory had been accurate. 2dly, That the contraction takes place even when the fluid does not cover the little glands. 3dly, That the con-

This theory
not plausible,

and inconsistent
with facts.

Conclusions
from them.

* Sennebier, *Physiol. Veg.* Tome V, p. 117.

traction

traction follows, although the foreign body is not brought into contact with all the hairs.

Apparent mistake of Roth.

This last conclusion is contrary to what is mentioned by Roth. He says, "if the insect be a small one, sometimes "only one edge of the leaf is folded up." *Hence it should seem necessary, that the insect should stir all the hairs of the leaf.*

Emptying the vessels not the cause of the contraction.

In the experiments mentioned no insect or any other body absorbs the fluid, and of course the vessels of the leaf cannot be emptied, which is completely in opposition to Broussonnet's theory.

Sennebie's hypothesis.

We shall next quote Sennebie's own opinion with regard to the contraction of the leaves of the *droseræ*. He says; "The hairs of the flowers" (he certainly means leaves) "of the *droseræ* are put in motion by a hair, a needle, an ant, or small bit of wood. It appears then, that the pressure alone is the cause of it, and this effect permits us to ascribe it to a cause purely mechanical *."

His facts true,

I am willing to agree with the first part of this sentence, but from the latter I must entirely dissent. Sennebie seems sensible, that the contractions of the leaves take place even when light bodies are placed upon them, which of itself would even lead us to suspect, that pressure is not alone the cause. I know, that, if we press on the centre of the leaf with a pin &c., we may cause its margin to approximate the pin; and this certainly would be owing to a mechanical cause. But suppose we see the contraction take place, as I have done, when a body specifically lighter than the leaf itself is placed in the centre, as a bit of rotten wood; should we be still inclined to ascribe it to a mechanical cause? Admit that it is the case. Suppose then we place the same bit of wood on the margin of the leaf, what effect ought to follow? If it was owing to a mechanical cause, or the weight of the forcing body, as in the last mentioned case, then we should expect, that the part of the margin of the leaf, on which the bit of wood rested, would be depressed; which undoubtedly is not the case, but on the contrary the margin rises, and then contracts toward the foreign

* *Physiol. Veg.* tome V, p. 104.

body,

body, or toward the footstalk of the leaf*. This would seem then to prove, that pressure alone can never be the cause of the contraction of the leaves of the *droseræ*, and consequently, that the action is not owing to a cause purely mechanical.

Having now seen, that the action of these leaves cannot be accounted for either by the theory of Broussonet, or that of Senneber, to what must we ascribe them? It appears to me, that the motions of these leaves must be owing to some other cause, and this cause a moving one†, denominate it what you will, for we must admit, that these leaves contract in consequence of the application of a stimulus; and I conceive, that this action is performed, if not by muscles, at least by *something* which is equivalent to muscles in the animal body.

I have seen no other attempts to prove; that the contraction of the leaves of the *droseræ* is owing to any mechanical action; and other authors seem disposed to admit it as a proof of vegetable irritability. Among these authors we have the illustrious Dr. Smith, who seems to think, that the motions of the leaves are to be explained on the principle of irritability‡. We have also the no less celebrated Wildenouw, who, immediately after mentioning the irritability of the *mimosa*, *dionœa*, &c., says: "Less conspicuous, but easily demonstrable, is the irritability of the indigenous species of sundew, *drosera rotundifolia* and *longifolia*§." I will now conclude this part of the paper by quoting the words of Dr. Smith, which he uses when speaking of the *mimosa*, &c.: "it is vain to attempt any mechanical solu-

* That this motion does not depend on pressure may be still better illustrated, by placing a fly, or some other body, on the apex of a leaf of the *drosera longifolia*. The hairs near the foreign body will contract around it, and then the apex of the leaf will rise upwards, and turn inwards, until it touches the base. Or if the offending body is small, the leaf will become convoluted around it.

† I mean a cause, which produces motion.

‡ Philos. Trans. abridged, vol. XX^l, page 243.

§ Princip. Botany and Veg. Physiol. (Translation) page 222, edit. 1805.

tion"

"tion of the phenomena mentioned". (Introduct. to Botany.)

What is the
the final cause
of this power?

Having found, that the leaves of the *drosera* catch flies on the principle of irritability, it may be asked, what is the intention of nature in allowing these and a few others that exclusive power? At present I am afraid this question cannot be satisfactorily answered; but in pursuing the inquiry we ought first to fix whether the particular contrivances in the *dionœa*, *drosera*, &c., are intended for offence or defence. Darwin entertained the latter idea; for, after mentioning the *silene*, he says: "In the *dionœa muscipula* there is a still more wonderful contrivance, to prevent the depredations of insects." Again, when speaking of the *droseræ*, he remarks, that, "This mucus is a secretion from certain glands, and, like the viscous material round the flower stalks of *silene* (catchfly), prevents small insects from infesting the leaves."

Darwin sup-
poses it intend-
ed to keep off
insects.

More probably
for the purpose
of catching
them.

I should rather be disposed to think, that the leaves of the *dionœa* and *droseræ* were intended for offence, i.e. for catching flies; for if defence was wanted, nature, ever simple in her operations, could have supplied these plants with a much simpler apparatus, as a number of spines, which would have been quite sufficient for this purpose; while we cannot conceive any contrivance, that would have answered better for catching flies, than what is seen in their leaves. Reasoning from analogy, this position will be strengthened. The *sarracenia purpurea* has tubular leaves beset at the margin with inverted hairs, which, like the wires of a mouse trap, render it very difficult for any unfortunate fly, that has fallen into the watery tube, to crawl out again. Now had it been the intention of nature, that this contrivance was for defence—would it not have been much easier for her to have placed the hairs on the margin of the leaf with the points upwards, instead of inverted, which would have effectually prevented the insect even from touching the inside of the leaf?

Leaves of the
drosera well
adapted for
this.

Regarding then these contrivances for offence, we have found the structure of the leaves of the *droseræ* admirably well calculated for this purpose. The glands are covered with

with a viscid fluid (probably) not only for alluring the little insects, but also for retaining them, until the contraction of the hairs (which is not immediate) shall begin. Now if the insect has been unable to overcome the tenacity of the fluid, it will soon be imprisoned by the hairs bending over it, and finally will either be killed by the contraction of the leaf, or retained in it until it dies. This contraction will continue until all is quiet, and even until the leaf becomes accustomed to its action, and of course suffers no farther stimulus; then most of the hairs and the leaf will expand and resume their former situations.

This is the manner then by which the flies &c. are imprisoned; but it may be inquired also, of what use are the insects to the plants? I think there can be little doubt, but that they are of some important use in the vegetable economy, or why should so many thousand insects be thus destroyed? Dr. Smith in his introduction to botany, after mentioning an interesting circumstance concerning the *sarracenia adunca* and *purpurea*, says: "Probably the air evolved by these dead flies may be beneficial to vegetation." And again: "probably the leaves of the *dioncæa muscipula*, as well as the *droseræ*, catch insects for a similar reason." On this subject I can say nothing at present, but must think Dr. Smith's explanation very ingenious, and probably just: but I cannot avoid asking one question, viz. As the flies in course of time are reduced to a pulpy state, both in the *dioncæa muscipula*, and in the *droseræ*, is it not probable, that some of the pulpy mass may be absorbed, and so prove as useful to the plant as the putrid effluvia?

Of what use is the insect to the plant?

Perhaps absorbed as nutriment.

P. S. Since the above observations were written out, about five weeks ago, two papers on the motions of vegetables have appeared. The 1st in a supplement to the 23d vol. of Nicholson's Journal, and the 2d in the 107th number, or that for the present month. The author of both is Mrs. Ibbetson, whose knowledge, industry, and perseverance deserve the highest encomiums. She endeavours to explain not only the motions of plants, but also their sleep, their sensibility, and their volition, by the changes produced

Mrs Ibbetson's theory

questioned.

duced upon the spiral wires (before denominated spiral vessels), and what she calls a leatherlike substance, by the actions of heat, light, and moisture. I must confess, that Mrs. Ibbetson has brought forward some strong proofs in confirmation of her opinion; but at the same time I must acknowledge, that these proofs are not sufficient to convince me, "that all plants are merely machines governed by light and moisture, and that every idea of their sensibility or of their volition, is only a proof, that we too often let our imagination run away with our judgment;" which is the opinion of Mrs. Ibbetson. On the contrary I am still inclined to believe, that plants are both sensible and irritable. As to volition, I avoid saying any thing of this at present. In prosecuting this inquiry, it must be considered, that plants are living organized bodies; and of course, that they are at least governed by the laws of vitality, if I may so express myself. No mechanical machine is governed by such laws. Mrs. Ibbetson's opinion with regard to the motions of the *mimosa sensitiva* is certainly different from that which I entertain; for admitting all the mechanical structure mentioned, consisting of "different joints, pulleys, knots, and bolts," to exist in the moving parts of plants, and that its spiral wires are capable of producing some of its motions; yet I cannot conceive, that either heat, light, or moisture, can possibly regulate some of the beautiful and striking experiments, which may be made either on the *mimosa sensitiva*, *m. pudica*, or others. Indeed such a mechanical structure seems to approach too near to the feeble works of men, and appears to me too complex (reasoning from analogy) to be the production of the *author of nature*. It is proper here to remark, that Mrs. Ibbetson's observations are mostly microscopical, and hence I am induced to suppose (though with the greatest deference to Mrs. Ibbetson's superior abilities) that possibly there may be some deception. But as I shall probably take the liberty of addressing a few observations on this important subject to Mr. Nicholson, after she has finished what she intends to write. I must for the present decline saying any thing farther; except, that, should Mrs. Ibbetson well explain these observations, I shall then be ready

ready to retract my opinion, and with the greatest pleasure give to her the merit of having explained that which has puzzled many physiologists.

On the Irritability of the Vessels of Plants.

As almost every vegetable physiologist has treated of the ascent of the sap, and as the irritability of the vessels is intimately connected with this operation, this subject becomes extremely interesting. Van Marum, in a paper addressed to Ingenhousz which is contained in the *Journal de Physique* for September 1792, notices some experiments, which had been made by Coulon, and then proceeds to mention some electrical experiments, which he himself had made on plants. He first makes a few observations on the destruction of the irritability of the muscular fibres, and then reasons thus: If the contraction of the vessels of plants is the effect of their irritability, it will be destroyed in the same manner as the irritability of the muscular fibres. He adds: "I tried if this would happen in the summer of last year upon some species of euphorbia, which have the common property of giving out much milky sap from their wounds. I caused the stream of the grand Teylerian machine to pass through the branches of euphorbia lathyrus, and through the twigs of euphorbia campestris, and cyparissias, and I observed, that all the branches or twigs of these plants, which conducted the stream or the electrical torrent during twenty or thirty seconds, absolutely when they were cut did not give out any more sap from their wounds. I repeated these experiments with the branches of the fig-tree, which also gave out milk by their wounds. The effect was perfectly the same; the sap was not seen to flow out when the branches were cut, after they had conducted the electrical torrent during five seconds; but when the electrified branches were pressed between the fingers, a little sap could be perceived to flow, which rendered it evident, that the electrical torrent had not emptied the electrified vessels, by forcing the sap towards the roots,

Ascent of the sap connected with the irritability of the vessels.

This irritability, like that of muscles, destroyed by electricity.

Sennebie
supposes this to
be owing to a
destruction of
the organiza-
tion :

“ but that the vessels had really lost the faculty of contract-
“ ing and of expelling the sap which they contained.”

Sennebie, to account for these phenomena, observes, that
“ electricity stops the passage of the juices in the branches
“ exposed to its action by the shock which it occasions in
“ them, which may derange their motions, and produce
“ some change in the juices themselves. However strong
“ the sparks, which suspend the flowing of the juices, I
“ have suspected, that this suspension was produced by the
“ disorganization of the parts of plants which experienced
“ its action, and that the extravasated juices were diffused
“ into the spongy parts, which retained them. I commu-
“ nicated this suspicion to Van Marum, who answered me,
“ that an electric torrent could not destroy any thing by
“ passing through a less perfect conductor, such as plants
“ are, but especially when it was divided in such a manner
“ that the light of the electric fluid could not be perceived;
“ yet I insisted on his recalling to mind, whether he had
“ seen any manifest disorganization in his experiments; and
“ I desired him to make the experiment, and observe the
“ electrified parts with a magnifying glass.” *Physiol.*
Vegetal. tome V, p. 111.

but this was
not the fact,

Agreeably to this request Van Marum repeated the ex-
periment, and answered Sennebie, that he saw no apparent
rupture (disorganization) in the organs of the vegetable.
Sennebie then confesses, that he regards the experiments
of Van Marum as the most favourable argument for the ir-
ritability of vegetables, and as the only one against which
he has nothing to oppose.

The milky
juice of certain
plants not their
sap, but a pe-
culiar secre-
tion.

In setting out with experiments of this kind it is neces-
sary to know, that what has been called the sap by both the
authors quoted is now thought to be a peculiar secretion.
Dr. Smith, in his introduction to botany, has placed the
milky juice both of the fig and spurge along with the se-
creted fluids of plants; and mentions, that Dr. Darwin has
shown this fluid, quite distinct from the sap, to be, like ani-
mal milk, an *emulsion* or combination of a watery fluid with
oil or resin.

If the vessels
containing it

Of whatever nature the juice of the euphorbia is, I do not
mean at present to inquire; for this fluid must be contain-
ed

ed in vessels, and if we can prove, that these vessels are irritable, then we can very easily transmit the analogy to the sap vessels. The experiments of Van Marum go far to establish the irritability of the vessels, which contain that secreted fluid; but the following, which I have made more than once, appear to me conclusive on the subject.

Having a number of plants of the *euphorbia helioscopia*, I cut off the top of one, and found, that the milky juice flowed copiously. I now submitted this plant to the electric influence for some seconds by passing sparks * through it, which were so small as seldom to be visible. I then cut the stem about the middle. and but very little juice flowed. I next covered the end of the remaining half with a little moss, and placed the root in water. For some days it seemed languid, but in a few more began to recover. Soon after this I cut the stem across about two inches from the root, and the milky juice flowed abundantly.

In this experiment then we find, 1st, that the milky juice was expelled by the contraction of the vessels. 2dly, That the electrical fluid weakened the irritability of the vessels, but did not destroy it, or kill the remaining half of the plant: and 3dly, That after a certain time, when the plant had recovered from the shock, the milky sap was again expelled by the contraction of the vessels. This experiment also shows, that Sennebier's suspicion concerning the disorganization of the parts of the *euphorbia helioscopia* is groundless.

I have repeated some of Van Marum's experiments with the same results as himself. The last mentioned experiment, is rather a delicate one, for it is difficult to regulate the electric stream so as that it will only hurt the irritability of the vessels without killing the plant; but should it prove successful in the hands of others, I should then be disposed to think, that the irritability of the vessels, which contain this secreted milky fluid, will be established on a sure foundation never to be overturned.

* Or rather the stream.

IV.

Demonstration of the Cotesian Theorem. By Mr. P. BARLOW.

To Mr. NICHOLSON.

SIR,

Oct. 13th, 1809.

Cotesian theorem.

IF the following demonstration of the celebrated theorem of Cotes appear to you to possess a sufficient degree of originality, to entitle it to a place in your Journal, its insertion will oblige

Yours, &c.

Royal Military Academy,
Woolwich.

P. BARLOW.

La Grange's conjecture of what led Cotes to it, suggested the demonstration here given.

I was led to the consideration of this theorem from an observation made by La Grange, in his *Théorie des Fonctions Analytiques*, where he hazards a conjecture as to the probable circumstances that led Cotes to the discovery of this elegant property of the circle; and by pursuing the hints there given I arrived at the following demonstration, which appears to me to be, at least, as satisfactory as any one that I have at present seen; and on this I rest my apology for intruding into the pages of the Philosophical Journal a subject, that has so long, and so often, engaged the attention of many very celebrated mathematicians, without any of them having arrived at what may be considered an unobjectionable demonstration.

It will be proper, however, for the information of some of your readers, before we enter upon the demonstration, to state the theorem itself, which is as follows.

Cotes's Theorem.

Theorem.

Let ABC , &c., Pl. VIII, fig. 31, 32, be any circle, divided into any even number of equal parts, $2m$, as AB , BC , CD , &c.; also let P be any point in the diameter, either

either within the circle, or in the diameter produced, and join PB, PC, PD, &c., then will

$$\begin{aligned} PB \times PD \times PF \text{ \&c.} &= A O^m + P O^m \\ \text{and } PA \times PC \times PE \text{ \&c.} &= A O^m \oslash P O^m \end{aligned}$$

Demonstration.

It is a well known trigonometrical property, that by Demonstration. making $\cos. x = p$, we may derive,

$$\begin{aligned} \cos. x &= p \\ \cos. 2x &= 2p^2 - 1 \\ \cos. 3x &= 4p^3 - 3p \\ \cos. 4x &= 8p^4 - 8p^2 + 1 \\ \cos. 5x &= 16p^5 - 20p^3 + 5p \\ \text{\&c.} \quad \text{\&c.} \quad &\text{See Bonycastle's Trig. p. 301.} \end{aligned}$$

Now by substituting $2p = y + \frac{1}{y}$, and multiplying each of the above formulæ by 2, they are reduced to the following simple forms:

$$\begin{aligned} 2 \cos. x &= y + \frac{1}{y} \\ 2 \cos. 2x &= y^2 + \frac{1}{y^2} \\ 2 \cos. 3x &= y^3 + \frac{1}{y^3} \\ 2 \cos. 4x &= y^4 + \frac{1}{y^4} \\ 2 \cos. 5x &= y^5 + \frac{1}{y^5} \end{aligned}$$

whence we may conclude generally $\left. \begin{array}{l} \text{whence we may con-} \\ \text{clude generally} \end{array} \right\} 2 \cos. mx = y^m + \frac{1}{y^m}$

But as this general form is only deduced from observing the law of the leading forms, it will be more satisfactory to see it derived in a direct manner: which may be done by means of the general formula.

$$\begin{aligned} \cos. nx &= 2 \cos. x \cdot \cos. (n-1)x - \cos. (n-2)x. \\ &\text{Bonycastle's Trig. p. 300.} \end{aligned}$$

Or

Demonstration of Cotes's theorem. Or making $n - 1 = m$, and transposing, we have

$$2 \cos. x \times 2 \cos. mx = 2 \cos. (m - 1) x + 2 \cos. (m + 1) x.$$

When, by making $2 \cos. x = y + \frac{1}{y}$, and admitting, that

in any case, $2 \cos. (m - 1) x = y^{m-1} + \frac{1}{y^{m-1}}$, and $2 \cos.$

$mx = y^m + \frac{1}{y^m}$, (which we know to be true when $m = 2$

from the above forms), we shall find that $2 \cos. (m + 1) x =$

$y^{m+1} + \frac{1}{y^{m+1}}$; that is, it will be of the same form with re-

gard to the multiple $m + 1$, which may be shown as fol-

lows. In the foregoing general formula, by substituting for

$2 \cos. x$, $2 \cos. (m - 1) x$, and $2 \cos. mx$, their respective

values $y + \frac{1}{y}$, $y^{m-1} + \frac{1}{y^{m-1}}$, and $y^m + \frac{1}{y^m}$; we have,

$$2 \cos. (m + 1) x = \left(y^m + \frac{1}{y^m} \right) \times \left(y + \frac{1}{y} \right) - \left(y^{m-1} + \frac{1}{y^{m-1}} \right)$$

the latter side of which equation being reduced gives

$$2 \cos. (m + 1) x = y^{m+1} + \frac{1}{y^{m+1}}$$

that is, it is of the same form with regard to its multiple as the two preceding forms; and since we know that those are true when $m = 2$, or when $m = 1$, it follows, from what is said above, that it is true when $m = 3$, and consequently also when $m = 4$, and generally for any value of m . We may therefore conclude with certainty, that if

$$2 \cos. x = y + \frac{1}{y}$$

$$\text{that } 2 \cos. mx = y^m + \frac{1}{y^m}$$

From which we readily deduce the following equations:

$$1^{\text{st}}, y^2 - 2y \cos. x + 1 = 0$$

$$2^{\text{d}}, y^{2m} - 2y^m \cos. mx + 1 = 0$$

Now

Now these equations must necessarily have one common root, being both obtained from the same value of y ; and farther, since both equations are reciprocal ones, if y be one root, $\frac{1}{y}$ is another root, and the first equation having but two roots, and such of those being also roots of the second equation, it follows that the former of our formulæ is a divisor of the latter. And this is true, if we change y into qy , without, however, altering the value of $2 \cos. \pi$, and $2 \cos. m\pi$; for by substituting for these their true value $y + \frac{1}{y}$ and $y^m + \frac{1}{y^m}$, and qy for y , the above formulæ are reduced to

$$\begin{aligned} \text{1st,} \quad & (qy^2 - 1) \times (q - 1) \\ \text{2d,} \quad & (q^m y^{2m} - 1) \times (q^m - 1) \end{aligned}$$

The former of which is evidently a divisor of the latter, entirely independent of the value of q , or of the measure of the angle represented by π .

We may therefore, instead of x , write $\frac{mx + nc}{m}$, then our formulæ will become

$$\begin{aligned} q^2 y^2 - 2qy \cos. \left(\frac{mx + nc}{m} \right) + 1 \\ q^{2m} y^{2m} - 2q^m y^m \cos. (mx + nc) + 1 \end{aligned}$$

The former being still a divisor of the latter.

We may here also observe, that while c represents the entire circumference, the $\cos. (mx + nc) = \cos. mx$, whatever integral value we give to n ; and hence making respectively $n = 0, 1, 2, 3, \&c, m - 1$, it follows, that the formula

$$q^{2m} y^{2m} - 2q^m y^m \cos (mx + nc) + 1,$$

has for its divisors the m following formulæ,

$$\begin{aligned} q^2 y^2 - 2qy \cos. \left(\frac{mx + 0}{m} \right) + 1 \\ q^2 y^2 - 2qy \cos. \left(\frac{mx + c}{m} \right) + 1 \\ q^2 y^2 - 2qy \cos. \left(\frac{mx + 2c}{m} \right) + 1 \end{aligned}$$

$$q^2 y^2$$

Demonstration
of Cotes's
theorem.

$$q^2 y^2 - 2 q y \cos. \left(\frac{m x + 3 c}{m} \right) + 1$$

&c.

$$q^2 y^2 - 2 q y \cos. \left(\frac{m x + \overline{m-1} \cdot c}{m} \right) + 1$$

Since then our first formula of the $2m$ th degree has for its divisors the above m formulæ of the 2d degree; it follows, from the nature of equations, that it is equal to the product of these m formulæ.

Now by making $m x = 0$, and $m x = \frac{c}{2}$, we have $2 \cos. m x = +1$, and the general form is reduced to this particular one,

$$(q^m y^m + 1)^2,$$

which is the Cotesian theorem.

For in the first case, making $m x = 0$, it becomes

$$(q^m y^m - 1)^2,$$

having for its divisors,

$$q^2 y^2 - 2 q y \cos. \left(\frac{0 c}{2 m} \right) + 1$$

$$q^2 y^2 - 2 q y \cos. \left(\frac{2 c}{2 m} \right) + 1$$

$$q^2 y^2 - 2 q y \cos. \left(\frac{4 c}{2 m} \right) + 1$$

&c.

&c.

And secondly, making $m x = \frac{c}{2}$, we have for our formula

$$(q^m y^m + 1)^2,$$

its divisors being

$$q^2 y^2 - 2 q y \cos. \left(\frac{c}{2 m} \right) + 1$$

$$q^2 y^2 - 2 q y \cos. \left(\frac{3 c}{2 m} \right) + 1$$

$$q^2 y^2 - 2 q y \cos. \left(\frac{5 c}{2 m} \right) + 1$$

&c.

&c.

Or using radius r , instead of radius 1, and writing $\frac{c}{2m}$ Demonstration of Cotes's theorem.
 $= x$, we have

$$\text{formula } (q^m y^m - r^m)^2, \text{ and divisors } \begin{cases} q^2 y^2 - 2qyr \cos. 0x + 1 \\ q^2 y^2 - 2qyr \cos. 2x + 1 \\ q^2 y^2 - 2qyr \cos. 4x + 1 \\ \text{\&c.} \quad \text{\&c.} \end{cases}$$

And also

$$\text{formula } (q^m y^m + r^m)^2, \text{ and divisors } \begin{cases} q^2 y^2 - 2qyr \cos. x + 1 \\ q^2 y^2 - 2qyr \cos. 3x + 1 \\ q^2 y^2 - 2qyr \cos. 5x + 1 \\ \text{\&c.} \quad \text{\&c.} \end{cases}$$

Now the first set of these divisors agrees with PA^2 , PC^2 , PE^2 , &c. in the above figures; and the second set with PB^2 , PD^2 , PF^2 , &c.

For since q is undeterminate, make $qy = OP$, $r = OB$, also $r \cos. x = OQ$; then by (Euclid 13, 2)

$$PB^2 = PO^2 - 2OQ \times OP + OB^2;$$

and it is exactly the same with all the other divisors.

Since therefore $(OB^m - PO^m)^2 = PA^2 \times PC^2 \times PE^2$ &c.

and $(OB^m + PO^m)^2 = PB^2 \times PD^2 \times PF^2$ &c.
 also $OA = OB$, it follows, that

$$OA^m \oslash PO^m = PA \times PC \times PE, \text{ \&c.}$$

$$\text{and } OA^m = PO^m = PB \times PD \times PF, \text{ \&c.}$$

Q. E. D.

V.

On the Influence of Electricity on Flame: by Mr. LEOPOLD VACCA, Colonel of the 32d Regiment of Light Infantry.*

FLAME has been much used in electrical experiments, but I do not know, that the effect produced by electricity on the figure of flame has ever been noticed. Effect of electricity on the shape of flame not known.

We know, that, if water dropping from a slender siphon be electrified, the dropping will be changed into a stream. Water dropping converted into a stream,

* Journal de Physique, vol. LXV, p. 224.

We

or made to
spout farther
by it.

We know too, that if a stream of water be electrified, its velocity will be increased, and it will describe a larger parabola. The reason of these phenomena is well known: they depend on the mutual repulsion of the particles of the water, all of them being electrified with the same kind of electricity.

Flame may be
supposed to be
enlarged by it:

Hence it might be expected, that, flame being an assemblage of very subtile particles, if we could introduce into it electricity of one kind, they must repel each other, and consequently the flame be enlarged.

but experi-
ment

To satisfy myself of the fact, I took a small vessel of metal filled with spirit of wine, and insulated it. By means of a metallic chain I formed a communication between the vessel and the conductor of a good electrical machine of glass. I kindled the spirit of wine without moving the machine, and observed the shape and magnitude of the flame.

shows the con-
trary.

I turned the handle of the machine, and perceived, that its action occasioned a very considerable contraction of the flame. When I suspended the action of the machine, I found, that the flame resumed its former dimensions. This experiment, a thousand times repeated, constantly afforded me the same results.

I was at first puzzled to account for this, but I think I have at length hit upon the true cause.

Electricity is-
suing from
points repels
them by its ac-
tion on the air;

We know, that electricity issuing out of a body to traverse the air repels it nearly in the same manner as powder repels the gun barrel in which it is burned. We know, that by means of points dispersing in the air the electricity accumulated in a star of metal very movable on a pivot we occasion the star to turn very rapidly in a direction contrary to that of the points. We know too, that on this principle Ferguson constructed a planetarium, which was set in motion by electricity. We know also, that no substance disperses electricity more than flame does.

and in the same
manner repels
the flame.

If then electricity escape from all the points that constitute the surface of flame, these points must be repelled back into the flame; consequently the flame will be compressed, and its volume diminished.

VI.

Of the Action of Phosphorus and oxygenized muriatic acid Gas on Alkalis; by Messrs. BOUILLON-LAGRANGE and VOGEL.*

SOME years ago we perceived, that, after having obtained a pretty large quantity of phosphuretted hydrogen gas from heating a mixture of phosphorus with pure caustic potash dissolved in water, a blackish substance remained; and toward the end of the process another gas was evolved, which no longer inflamed on contact with the air.

Phenomena of phosphorus heated with potash observed long ago.

This circumstance not appearing of sufficient importance for us to pursue it, we had thought no more of the memorandum made of it, and should probably have neglected it still, had not the same phenomena occurred again lately, as we were lecturing on phosphorus. Struck at the same time with the discovery of Mr. Davy respecting the nature of alkalis, we could no longer look with indifference on the facts we perceived anew.

Occurred again lately.

We do not mean to discuss before the Society the decomposition of potash by the processes, that Messrs. Thenard, Gay-Lussac, and Curaudeau have employed. We shall only observe, that we have repeated in presence of the pupils, who attend our chemical lectures, the experiment of the decomposition of soda by charcoal in a gun barrel, as we had seen it performed by Mr. Curaudeau†, and succeeded completely. The experiments we are about to submit to the Society were made with phosphorus on potash and soda. At present we shall content ourselves with exhibiting the facts as we observed them, carefully abstaining from all conjectural theory, persuaded, that, to attempt explanations not grounded on incontestable facts, or requiring interpretation, must give rise to errors, which, instead of advancing science, only tend to render our notions uncertain.

Soda decomposed by means of charcoal.

Experiments with phosphorus and alkalis.

* Annales de Chimie, May 1808, vol. LXVI, p. 194.

† See p. 57 of our present volume.

Preparation of phosphorus for mixing it with alkalis.

To obtain the mixture of phosphorus with potash, which appeared to us impracticable unless by the method we shall mention, we fused phosphorus in a phial into which we had put some warm water. The phial was shaken till the water was cold; to accelerate which we immersed it in cold water, after a little while, still continuing to shake it. Thus the phosphorus was reduced to the state of powder. The supernatant water being decanted off, it was replaced by diluted oximuriatic acid. This acid, we are taught by Mr. Juch of Wurtzburg, has the property of depriving phosphorus of carbon, if it be true that it contains any. From coloured it becomes white, and in this state the acid is to be separated from it, and the moisture absorbed by blotting paper.

Purity of potash best ascertained by barytes water.

On the other hand we satisfied ourselves of the purity of the potash by treating it afresh with alcohol, and, after fusion, testing it with lime water and barytes water. We shall here observe, that lime water is not a certain test for determining whether potash retain any carbonic acid; for, if the mixture be diluted with water, a small quantity of carbonate of lime will be dissolved. This solution does not take place with barytes, and the smallest quantity of carbonate of barytes is always visible, which renders this substance preferable to lime for examining potash or soda*.

Mixture of the phosphorus with potash.

Caustic potash was reduced to powder in a glass mortar, and then an equal quantity of phosphorus, prepared as above, was added. To avoid the combustion, which took place before the temperature was lowered, we placed the mortar in a mixture of powdered ice and muriate of soda. A slight trituration was sufficient, and the mixture was immediately introduced into a coated stone retort, which was

Caustic potash precipitates from lime water carbonate of lime that is soluble.

* It has long been known, that a concentrated solution of caustic potash is precipitated by lime water, and that this precipitate is soluble in a large quantity of water; whence it has been inferred, that the potash merely took the water from the lime, and the latter fell down in the caustic state. But we have found, that this precipitate is in fact a carbonate of lime, which, thus intimately divided, is soluble in water. We have observed too, that this solution is not owing to an excess of alkali, for, after passing carbonic acid gas into lime water, the precipitate separated was equally soluble in water, yet the liquor was neutral.

placed

placed on the grate of a reverberatory furnace. A tube of safety was fitted to the beak of the retort, which communicated with a jar filled with mercury. The whole being thus arranged, a gentle heat was applied. This first degree of heat sometimes occasioned the combustion of a small portion of phosphorus; but this may be prevented by covering the mixture with a little powdered potash. It is easy to conceive, that this combustion is owing to the air contained in the retort; and that, when the caloric has occasioned a vacuum in the apparatus, no combustion can take place. Of this we satisfied ourselves by a direct experiment. We afterward increased the fire, till the retort was of a white heat.

During the whole course of the process, a gas was evolved, the properties of which we shall mention presently.

When the retort was completely cold, we broke it, and found in it a black mass. Its inside was entirely covered with a coat shining as if metallic, and having the appearance of carburet of iron.

The black matter has a slightly alkaline taste, and but little soluble in cold water. By means of boiling however we dissolved it all, except a black powder, which was precipitated. Boiling nitric acid likewise dissolves it; and a black matter, which is nothing but oxide of carbon, separates in a similar way.

Neither of these solutions contains any thing but phosphate of potash.

Among the various experiments we made there was one, in which we obtained a similar black mass, but without any perceptible taste. Water had no action on it. Nitric acid dissolved it, and separated from it oxide of carbon. The portion of the tube communicating with the retort was lined with a grayish substance, which took fire on coming into contact with water. As to the salt that remained in the retort, it was nothing but neutral phosphate of potash, which we know to be nearly insoluble in water.

In the course of these experiments we employed alternately potash and soda, and instead of a stone retort, a retort and tube of porcelain. The results were the same.

The

Properties of
the gas.

The properties exhibited by the gas mentioned above were: 1. It was neither acid nor alkaline. 2. It had a slight alliaceous smell. 3. It took fire at the approach of the white flame of a taper, and formed by this combustion a little phosphoric acid and oxide of phosphorus. 4. It detonated loudly, when mixed with oxygen gas, and touched by a substance in the state of ignition. 5. It did not take fire on coming into contact with the atmosphere, with oxygen gas, or with nitrous gas. 6. It was a little soluble in water; and in this solution nitrate of silver occasioned a blackish precipitate. 7. It inflamed rapidly when mixed with oximuriatic acid gas, and afterward deposited a little oxide of phosphorus on the sides of the jar.

Easy method
of procuring
this gas.

This elastic fluid may be procured in a simple and easy way. It is sufficient to put a little phosphorus, cut into small pieces and very dry, into a common phial; to strew over it perfectly dry caustic potash; and to adapt to it a curved tube opening under a jar filled with mercury. On heating the phial gently white vapours will form without inflammation, and the gas will be evolved. The temperature is to be raised gradually, till no more bubbles pass. There will remain in the phial a black substance, slightly alkaline, containing phosphate of potash.

Residuum.

Difference of
the results if
water be pre-
sent.

There is a very striking difference when a little water is added to the mixture. As long as any moisture is present, we obtain phosphuretted hydrogen gas, which inflames on the contact of air; but as soon as the matter is dry, if the action of the fire be continued, the gas evolved no longer inflames by the contact of air, and has all the properties of that above mentioned.

This difference in the results no doubt deserves examination, and perhaps may be explained without any hypothesis. The same may be said of the following experiment, which may serve to elucidate the phenomena above described.

Oximuriatic
acid gas passed
through potash
at a white heat

Two drachms of pure potash were introduced into a porcelain tube passing through a reverberatory furnace. Through this tube, brought to a white heat, was transmitted oximuriatic acid gas, expelled from a matrass into which the proper ingredients had been put. An intermediate phial,

phial, without water, received the gas before it reached the porcelain tube; and the other extremity of the tube communicated with a pneumatico-chemical apparatus.

The moment the oximuriatic acid gas had reached the potash, a great deal of water passed into the jar in vapour not easily condensed. When condensed they left behind carbonic acid gas. Some time after oximuriatic acid gas was perceived in the jar. On examining it a copious precipitate was obtained with lime water and barytes water, but it was necessary to employ them in excess. Toward the end of the process no more oximuriatic acid gas passed over, but a mixture of oxygen and carbonic acid gas.

Aqueous vapour first passed, then oximuriatic acid gas, and lastly oxygen, each mixed with carbonic acid.

Carbonic acid gas therefore was disengaged during the whole course of the operation, taking place at its three different periods; first with the water in vapour; secondly with the oximuriatic acid gas; and thirdly with the oxygen gas. All these gasses were cloudy, and did not become transparent till the water was deposited.

The quantity of carbonic acid gas, collected and separated, appeared to us too great to be ascribed to the acid, that the potash retains. Besides, we employed an alkali we had carefully purified; and the acid it could contain, for whatever precaution is taken it cannot be perfectly freed from it, was only to be detected by barytes water, which gave only a slight cloud, scarcely perceptible.

Too much carbonic acid to have been retained in the potash.

We have not however any design, still less the presumption, to attempt to establish or determine the principles of potash: though from this experiment we might be tempted to suppose, that hydrogen and carbon exist in this alkali in certain proportions.

Probably the alkali contains carbon and hydrogen.

In the porcelain tube we found muriate of potash in thin white laminæ but slightly adherent. Some of them were tinged of a light green. The weight of this salt was much inferior to that of the potash employed.

Muriate of potash in the tube less in weight than the alkali employed.

It follows then, that all these experiments, if of little importance in themselves, may lead us to examine with more attention the changes, that take place in substances, when placed in contact with others at temperatures more or less elevated.

Potash treated
with oxygen &
hidrogen gas.

The action of oxigen gas and hidrogen gas on potash has likewise presented us with some phenomena, of which we shall give an account.

VII.

On the Chemical Analysis of the Onion: by Messrs. FOURCROY and VAUQUELIN.*

Liliaceous
plants similar
in structure,
yet widely dif-
ferent in pro-
perties.

AMONG the plants that compose the very natural family of liliaceæ, and seem to have the same interior organization, there are some, which, as the onion, differ essentially in their taste, smell, and almost all their properties. The authors of the paper, of which we shall here give an abridgment, had in view to seek the causes of this difference. Beside the solution of this problem, their investigation includes several chemical facts of great importance to the progress of vegetable analysis.

Properties of
the juice of the
onion.

The onion, *allium cepa*, grated to a pulp, and subjected to the action of a press, gives out a white viscid juice, somewhat opake, of a strong smell, colourless the instant it is filtered, but acquiring a rosy hue from the contact of the air, on account of the oil it contains. It is perceptibly acid. It is precipitable by the acetate of lead, lime, oxalic acid, nitrate of silver, and potash. Distilled it yields a milky water, slightly acid, with a few drops of oil floating on the surface.

The water dis-
tilled from the
juice contains
sulphur.

The water distilled from onion juice has a strong smell, and forms a light yellow precipitate with acetate of lead. Two experiments were sufficient to prove the existence of sulphur in this liquor. 1. Oximuriatic acid makes it clear, deprives it of its smell, and gives it the property of precipitating nitrate of barytes. 2. This liquor, distilled in a copper alembic, forms on the surface of the head a black iridescent pellicle, which is sulphuret of copper. The sulphur is held in solution in the onion juice by an essen-

* Annales de Chimie, vol. LXV, p. 161. Abridged by Mr. Laugier from the original paper read to the National Institute.

tial oil, which exists in it with a small quantity of acetous acid.

The part of the juice left in the retort deposited a fawn coloured matter, having a strong smell of onions. From this sediment alcohol separated oil and sulphur. What was not acted upon by the alcohol yielded by distillation a black, fetid oil, and carbonate of ammonia, which indicate the presence of a vegeto-animal matter, in the coagulum of the onion juice. Examination of the juice left after distillation.

The fluid from which the preceding sediment was separated had a deep brown red colour and a saccharine taste. With acetate of lead it gave a yellow precipitate. This precipitate, heated before the blowpipe, grew black, emitted a smell of sulphurous acid, and left a globule of phosphate of lead. The solution of the residuum in sulphuric acid diluted in water, heated, and filtered, yielded two precipitates of phosphate of lime on the successive addition of ammonia and lime water. Hence the authors conclude, that the precipitate formed by acetate of lead in distilled onion juice is composed of oxide of lead, phosphoric acid, sulphur, and a vegeto-animal matter.

Messrs. Fourcroy and Vanquelin, having employed fermentation as a good mean of vegetable analysis on several occasions with success, tried it with onion juice. Exposed to a temperature of 15° or 20° [59° or 68° F.] in a suitable apparatus, this juice emitted no gas; but it acquired in succession a tint of rose colour and of yellow, and a fawn coloured sediment was deposited. The vessels being unluted, they were surprised to find, that the juice was converted into vinegar, but that it retained the onion smell as strong as before fermentation. This proves, that the volatile or essential oil had undergone no alteration. They afterward found, that, if the alcoholic fermentation did not take place, it must be ascribed to the absence of a suitable ferment. Onion juice examined by means of fermentation.

The sediment formed during the acetous fermentation of the juice appeared to demand particular attention. This substance has the following properties. *a.* It is in a state of minute division, forms a smooth paste, and has a strong onion smell. *b.* Alcohol takes up from it sulphur and oil, Examination of the sediment formed during the acetification of the onion juice.

as is easy to judge by the action of oximuriatic acid, which renders the alcohol turbid, and communicates to it the property of forming a copious precipitate with nitrate of barytes. *c.* After being treated with alcohol the sediment has less smell: it scintillates on burning coals, shrivels, and then swells up, emitting the fetid smell of animal substances. *d.* Mixed with a solution of sugar, no movement was produced, and no alcohol was formed: whence we may conclude, that this substance is not of the nature of yeast, and not calculated to excite alcoholic fermentation.

Examination of the vinegar of onion juice, and the crystallizable matter it holds in solution.

The vinegar formed by onion juice had a yellowish colour, a very strong smell of onions, and an acid taste, but yet saccharine*. It marked 6° on the areometer for acids: but this density was owing to a peculiar substance, which gives it the property of crystallizing when it is sufficiently concentrated.

This substance, which particularly excited the attention of Messrs. Fourcroy and Vauquelin, is neither an acid nor a neutral salt. It presents itself in the form of fine, white, acicular crystals, disposed in diverging rays: it has a saccharine and at the same time acid taste: it is mixed with a gummy matter, and also with citric acid. Hot alcohol dissolves both the crystalline substance and the acid accompanying it, leaving the gummy matter untouched. As the alcoholic solution cools, white needle crystals separate, shining, and arranged in stars.

Properties of the crystals.

These crystals have the following properties. *a.* They are of a snowy whiteness, and of a mild, saccharine taste. *b.* They are equally soluble in water and in alcohol. *c.* They burn like common sugar. *d.* Their solution does not ferment with yeast. *e.* Nitric acid converts them into the oxalic. They afford no mucous acid, unless when they contain mucilage. Our authors satisfied themselves on this occasion, that manna, with which they compared them, is wholly converted into oxalic acid, and does not yield an atom of mucous acid, on being treated with the nitric acid, if care be taken to separate all the mucilage that accompa-

* Pickled onions, when long kept, perhaps two or three years, acquire a saccharine taste, so as at length to lose almost all their acidity. C.

nies it. From these experiments they infer, that the crystalline matter of onion juice is nothing but manna. This substance is manna.

It remained now to determine, whether the manna were ready formed in the juice, or developed by fermentation. formed by the fermentation.

To solve this question, they treated onion juice concentrated by evaporation in several ways, and obtained only fermentable sugar, instead of the manna which the fermented juice had furnished. It appears then, that the manna obtained from onion juice is the product of fermentation; and this opinion is the more probable, as a scrupulous examination of the fermented juice exhibited to them all the principles it contained before, except sugar*.

From the preceding experiments Messrs. Fourcroy and Vauquelin conclude, that sugar, either when its solution is too dilute, or when it contains a different ferment from yeast, constantly undergoes a kind of alteration by acetification, which divides it into two new compounds, unequal in quantity, and differing in the proportion of their principles: one vinegar, which contains fewer radicals than sugar; the other manna, which contains more radicals than sugar. In fact, all the chemical knowledge we have of these three substances confirms this result.

Perhaps, add the authors, there is no improbability in supposing, that, in the trees which furnish manna, this substance is formed in their saccharine juice by the acetous fermentation of sugar, assisted by the glutinous matter that exists in all vegetables. It is natural to suppose, that the saccharine juice of the ash and the birch, once escaped from its vessels, runs into the acetous fermentation; and that the results are manna and vinegar, the latter of which afterward evaporates. This no doubt is the reason, why new manna is acid, and smells of vinegar. This opinion may be confirmed by examining the kind of sap or liquor, that flows from trees apt to furnish manna, when the stem is tapped. Natural formation of manna.

The examination, that Messrs. Fourcroy and Vauquelin made of manna, convinced them, that beside the crystallizable matter analogous to what they obtained from the fer- Examination of natural manna.

* The fact mentioned in the preceding note tends to confirm this. C.

mented onion juice, this substance contains a small quantity of fermentable sugar, which was observed by Proust and Thenard; also a small portion of yellow matter of a nauseous smell and taste, which fermentation does not destroy, and to which they think its purgative quality is to be ascribed; and lastly a little mucilage, which alone is converted into mucous acid when manna is treated with nitric acid. Melon-juice in like manner affords them manna, which they could not discover previous to fermentation.

Spirituos fer-
mentation of
onion juice.

Desirous of knowing whether onion-juice, as a saccharine liquor, be capable of affording alcohol on the addition of a suitable ferment, our authors mixed 244 gr. [3767 grs.] of this juice, reduced to the consistence of an extract, with 2 lit. [2 wine quarts] of water, and 30 gr. [463 grs.] of beer yeast of the consistence of paste. The mixture, exposed to a temperature of 16° or 20° [61° to 68° F.], exhibited all the phenomena observed during alcoholic fermentation. Carbonic acid was evolved; and the distillation of the fermented liquor yielded 134 gr. [2069 grs.] of brandy at 22°, equivalent to 73 gr. [1127 grs.] of alcohol at 40°. This quantity of alcohol, according to Lavoisier, requires for its production 114 gr. [1760 grs.] of sugar.

General results
of the analysis
of the onion.

From the experiments above related it follows, that the onion is composed of, 1, a white, acrid, volatile, and odorous oil: 2, sulphur combined with oil, which occasions its fetid smell: 3, a large quantity of uncrystallizable sugar: 4, a great deal of mucilage analogous to gum arabic: 5, a vegeto-animal matter coagulable by heat and analogous to gluten; 6, phosphoric acid, partly free, partly combined with lime; and acetic acids: 7, a small quantity of citrate of lime, which had never before been met with in vegetables: 8, a parenchymatous or very tender fibrous substance retaining vegeto-animal matter.

Sources of its
properties.

It is to the combination of the oil of the onion, the sulphur, the saccharine substance, and the mucilage, that we must ascribe the emulsion or milk, that flows from the slices of this bulbous root, its acrimony, its property of irritating the eyes, exciting tears, blackening silver, &c. Most acrid plants, as the euphorbias, chelidonias, arums, hellebores, owe their injurious qualities to oily and resinous substances:

Acrimony of
plants resides
in an oil, or re-
sin, and best

stances:

stances: and the authors recommend the oximuriatic acid destroyed by oximuriatic acid, as the most certain antidote, to destroy the pernicious effects of this acrid principle.

The presence of free phosphoric acid in plants is an interesting fact, but how is it produced there? Does it pass Free phosphoric acid, how gets it into plants? directly from the earth into plants? or does it come from phosphorus absorbed by the plant from the soil? Of these two questions the authors have sought the solution; and various arguments, supported by accurate observation, have led them to think, that the phosphorus existing in the animal matters employed to promote vegetation passes in combination with fats and oils into the plants, where it combines with oxygen, and produces the phosphoric acid we meet with.

Messrs. Fourcroy and Vauquelin terminate their paper by Analyses of common plants advantageous. very judicious reflections on the advantages, that may be derived from analyses of the plants that are most common and most in use. The numerous and interesting facts contained in their paper, and the consequences deduced from Some caleuli perhaps soluble by onion juice. them, among which we must not omit the possibility of the solution of earthy phosphoric caleuli by the juice of onions, leave no doubt of the benefit that may accrue from researches of this kind, in promoting vegetable chemistry, and the knowledge of vegetables in general.

VIII.

Abridgment of a Paper on the Species of Carnivorous Animals, the Bones of which are found mixed with those of Bears in Caverns in Germany and Hungary. By Mr. CUVIER.*

1. **O**N a separate paper on the fossil hyena, I have already mentioned, that bones of this animal were found in the Baumannshoehle, and in a cavern at Gaylenreuth. Out of a quantity of bones from the latter, among which those of bears were most numerous, I procured a jawbone of a Bones of the hyena found with those of the bear.

* Journal de Physique, vol. LXV, p 282,

hyena,

hyena, more complete than those I had before represented, but exhibiting precisely the same characters. The whole of the lower edge and the condyloid process are very perfect; and the four jaw teeth are seen, but a little broken. The anterior extremity and coronoid apophysis only are wanting.

The four jaw teeth occupy the length of 0·092 m. [3·6 inches] nearly the same space as in the piece from Fouvent, a place in Franche-Comté, where fossil bones of the hyena are found.

Another fragment from the same place is part of the jaw bone of an hyena, which must have been larger than the great hyena of the Levant in the proportion of 3 to 2.

Lastly Mr. Blumeubach sent me a drawing of the fourth or principal upper grinding tooth of an hyena found in the same place.

Bones of an
animal of the
genus felis,

2. A very large animal of the genus felis has also left numerous remains in these caverns. Proofs of this are found for those of Hungary in Vollguard's paper in the *Ephem. Nat. Cur.*, an. iv, dec. 1, obs. 170, p. 227. It is an ungicular phalanx, easily known by its great vertical height, little length, and different projections.

mentioned by
Soemmering,

Leibnitz in his *Protogea* has represented part of a fossil skull of an animal of this order found in the cavern of Schartzfels. Soemmering has given a more accurate delineation of the same specimen, which is at present in the museum at Goettingen. He asserts, that this cranium perfectly resembles that of a middle sized lion, and differs from that of the bear of the caverns in thirty-six particulars, which he points out: but most of these particulars are common to all the genus felis, and not peculiar to the lion.

Esper,

Esper has had engraved several teeth found in the cavern of Gaylenreuth, which closely resemble those of an animal of the felis genus, if we could depend on the accuracy of the engraving: but the differences between some of these teeth and those of the hyena depends on such slight variations, as might have escaped a common draughtsman.

and Rosen-
mueller.

Mr. Rosenmueller promises soon to publish a work, which will contain a description of the bones of an unknown animal
of

of the lion kind, and he adds, that "these bones are not precisely similar to those of the present lion."

In the mean time he gives us, without being aware of it, three bones of this genus, which he has suffered to slip in among those of the bear: namely the semilunar scaphoid, the cuboid of the hind foot, and the first cuneiform. But if these figures be of the natural size, the animal must have been of prodigious dimensions, which the other bones that I have examined do not indicate.

Indeed I have myself some new pieces to produce both from Gaylenreuth and other places. First single teeth. A second and third upper grinder of a felis: both from Gaylenreuth. Another from the cavern of Altenstein, with the drawing of which I was furnished by the celebrated Blumenbach. These teeth differ completely from those of the hyena.

I have likewise half a lower jaw from the collection of Mr. Hadrian Camper. It is that of a felis. The posterior tooth bilobated and without a heel, the vacuity before the alveolus of the last but one, the direction of the lower edge, and the situation of the maxillary foramina, leave no room to doubt of it.

But when the question is, to what species of felis does this half jaw come the nearest? the answer is not so easy. I will venture to say, that it is impossible without the numerous means of comparison, which I was so fortunate as to have in my power. Now these means have demonstrated to me, as they will to any one who shall employ them, that this bone belonged neither to a lion, nor lionness, nor tiger; still less to a leopard, or the little panther of the keepers of wild beasts: but that, if we must refer it to a living species, it can only be to the jaguar, or great spotted panther of South America, which it most resembles, particularly in the curve of its lower edge.

Probably belonged to the felis onca.

The most accurate ideas we have hitherto of the different large animals of the genus felis will perhaps occasion a doubt of this: but the characters of these animals and their osteology will be the subject of a separate dissertation, that will remove all the difficulties.

Bones of an animal of the genus *canis*.

3. The bones of an animal of the wolf or dog kind are the first I have found fossil, that are no way distinguishable from those of animals now inhabiting the surface of the same country : but then it is in a genus, where the distinction of species by separate bones alone is almost impossible.

Skeletons of many of these not easily distinguishable.

Daubenton has already observed how difficult it is to distinguish the skeleton of a wolf from that of a mastiff, or shepherd's dog of the same size. More interested than he in finding out their characteristics, I have studied them longer, carefully comparing the heads of several individuals of these breeds of dogs with those of several wolves. All that I have been able to remark is, that wolves have the triangular part of the forehead behind the orbits a little narrower and flatter, the sagitto-occipital ridge longer and more elevated, and the teeth, particularly the canine, larger in proportion. But these differences are so slight, that there are frequently much greater between individuals of the same species; and we can scarcely avoid thinking with Daubenton, that the dog and the wolf are the same species.

These noticed by Esper,

The existence of wolf's bones in the cavern of Gaylenreuth was announced by Esper in his first work. He gives a portion of the upper jaw, pl. X, fig. a, and three canine teeth, pl. V, fig. 3 and 4, pl. XII, fig. 1. He adds in his second paper, that wolves skulls of the common size have occurred almost as frequently as those of bears, mixed with those of dogs of the same size; and with others smaller.

Rosenmueller,

Mr. Rosenmueller too observes, that bones of the wolf kind occur at Gaylenreuth in the same state as those of the bear, and that they were deposited there at the same period.

and Fischer.

Mr. Fischer has sent me the drawing of one of these wolf's heads taken from Gaylenreuth, and preserved in the collection at Darmstadt. It is more likely the head of a wolf than of a dog by the elevation of the sagitto-occipital ridge : but if we may trust to the drawing, the face is not so long in proportion to the skull as in the common wolf, and the muzzle not so slender, to speak absolutely.

I would

I would recommend therefore to those, who have at their command any of these fossil skulls of wolves, to make a comparative examination of them attentively. With accurate measurements they might perhaps find some constant specific character. I have before me only lower jaws. Our museum possesses four, all from Gaylenreuth. I have a fifth from the same place, that was in Mr. Camper's collection.

A comparison
of the skulls
recommended.

All these pieces so nearly resemble the analogous bones in wolves and great dogs, that the eye can scarcely perceive any difference, even individually. The ascending branch however resembles the dog more than the wolf, because it is smaller in proportion, and the condyloid process is larger. The groove for the insertion of the masseter muscle is also narrower and deeper: but I repeat, these characteristics are so slight, that I cannot venture to offer them as distinguishing, if the analogy of other fossil bones did not authorize us to believe, that there were specific differences with respect to these also.

However, if these differences be not sufficiently proved, the identity of the species is not by this resemblance of some parts. The various species of the genus *canis*, the different foxes, &c., resemble one another so much in shape and size, that it is very possible some of their bones may not be distinguishable.

It is proper to observe here, that these bones, whatever they are, are in the same state as those of the bear, *felis*, and hyena; their colour, consistence, and covering are the same. Every thing indicates, that they date from the same period, and were buried together.

I have taken myself from a block of tufa filled with bones, a tooth, and a metacarpal bone of the thumb. The latter resembles in all respects that of a wolf or a large dog.

This species of wolf is found, as well as that of the hyena, with the bones of elephants. Mr. Jaeger has sent me the drawing of his most perfect lower jaw found at Cantstadt, and Mr. Camper that of a tooth of the same kind found at Romagnano, in the place where the elephants bones described by Fortis are found. Mr. Esper says too, that he

Found with
the bones of
elephants.

has

has some of these wolf's heads from Kahldorf, in the county of Eichstaedt, taken from the quarry where the hyena's head described by Collini was found, which I have mentioned elsewhere.

Bones of an animal resembling the fox.

4. We have also the bones of an animal very like the fox, if it be not the fox itself, at Gaylenreuth. Mr. Rosennueller thinks, that these, with the human bones, and those of the sheep and badger, are more recent than those of the bear, as they are in better preservation. It is possible, that there may be such, but to those I am going to mention this does not apply. They were embedded in the same tufa as the bones of the bear and hyena, from which I extracted them myself; and their composition is not less altered. If they be whiter, it is perhaps because, being smaller, the causes capable of depriving them of their animal matter acted upon them with more force.

Very abundant.

They must be very common there, for I took all those of which I am speaking from a block a few inches in diameter, composed in great part of bones of the bear and hyena: but they who have searched these caverns have been struck only with the large bones, and have neglected the smaller, which are neither less curious, nor of less importance towards the solution of the grand problem of fossil bones.

Enumeration of them.

My foxes' bones consist of the following: 1, an outer incisive tooth: 2, a canine tooth; both of the lower jaw: 3, an ungular phalanx: 4, an intermediate phalanx: 5, a first phalanx: 6, a phalanx of the imperfect toe of the hind foot: 7, a first metatarsal bone: 8, a cuneiform bone of the carpus: 9, a first cuneiform of the tarsus: 10, a second cuneiform of the tarsus: 11, a vertebra of the middle of the tail: 12, several sesamoid bones.

To this species I also refer the canine tooth represented by Esper, Pl. X, fig. c.

Compared with those of the common fox.

All these bones, compared with the analogous ones in the skeleton of a full grown fox, appeared rather larger. That of the metacarpus in particular was a little longer, without being larger: but these differences were not sufficient to establish a difference of species. On the other hand the different foxes, as the corsac, the isatis, or jackal, the Cape fox, *c. mesomelas*, and the two American foxes, *c. Virginianus*,

nus, and *c. cinereo-argenteus*, resemble each other too much in size, for us to suppose that these parts of the skeleton, which in general are not very characteristic, should exhibit greater differences than those observed in the bones of the fossil fox.

I would recommend it therefore to persons, who live near these caverns, to procure other bones of this species, and particularly skulls, that they may resume the comparison. As far as I can judge from an imperfect skeleton of a jackal, which I have examined, I should not be surprised to learn, that they resemble the bones of this animal more than those of our common fox. Further examination necessary.

5. The same block, that furnished me with the fox's bones I have just described, supplied me with some of a much smaller carnivorous animal of the weasel genus, and resembling the European polecat, or that of the Cape. These consist of 1, a portion of the pelvis including the pubis and ischium: 2, the two outer metatarsal bones: 3, a phalanx of the second row: 4, the last but one of the dorsal vertebra: 5, two caudal vertebrae. Bones of a species of weasel.

These are certainly bones of a weasel: and of all the skeletons of this genus I have had an opportunity of examining, there are only the polecats of Europe and of the Cape of good Hope, to which I can refer them. Most resemble those of the European or Cape polecat.

The martin and common weasel have the metatarsal bones in particular incomparably larger. In the zorilla and polecat they are exactly similar to the fossil specimens.

The dorsal vertebra is neither so long nor so large as in the polecat: but it resembles that of the zorilla; and this resemblance struck me particularly at first, as the bones of the hyena of the caverns also greatly resemble those of the spotted hyena, which is equally an inhabitant of the Cape. But the fragment of the pelvis directed me again to the polecat of Europe, which it resembles more than it does the zorilla. Thus I could not venture to lay down the hypothesis, which at first appeared so seducing, that we must search in the neighbourhood of the Cape for the animals most resembling those of our caverns.

It is extremely desirable, that more of these small bones should be collected, and compared also with those of the *mustela*

mustela Sarmatica, or polecat of Poland, of the *m. Siberica*, or yellow martin of Siberia, and of the Siberian polecat. I have never yet seen the skeletons of these three species*.

IX.

Account of some Colours for Painting, found at Pompeii: by Mr. CHAPTAL. Communicated to the First Class of the Institute, March the 6th, 1809†.

Paints found in a colour shop at Pompeii.

HER majesty the empress and queen has done me the honour, to put into my hands seven specimens of paints, found in a colour-shop at Pompeii.

Terra verte.

Of these one has undergone no preparation. It is a greenish and saponaceous clay, such as nature affords in various parts of the globe, and analogous to what is known by the name of *terra di Verona*, or *terra verte*.

Yellow ochre.

The second is a fine yellow ochre, which has been freed by washing, as is done in the present day, from all the matter injurious to its beauty or pureness. As this substance is reddened by calcination with a very moderate heat, it affords a fresh proof, that the ashes, by which Pompeii was overwhelmed, retained but a very slight warmth.

Spanish brown.

No. 3 is a brown red, of the same nature as that at present in our shops, which is employed for the coarse reddish coat applied to casks in seaports, and to the doors and win-

All these animals found in hot climates.

* One of the things that must appear at first sight the most astonishing in the collection of the fossil bones, with which these caverns are filled, is to find there bones of animals, which we should suppose could not live in the same climate: but it is possible, that all these animals may have existed in the same country. The animals of the genus *felis*, whether lions or tigers, indicate, that the country at that time must have enjoyed a pretty warm climate; and we know from unquestionable testimony, that the wolf, jackal, polecat, and bear, are all found in Africa likewise. J. C. Delam  therie.

† Annales de Chimie, vol. LXX, p. 22.

dows of some houses. It is produced by the calcination of the yellow ochre just mentioned.

No. 4 is a pumice stone, very light, and very white. It is Pumice stone, of a fine and close grain.

The other three are compound colours, which I have been obliged to analyse, in order to know their constituent principles.

The first of these, No. 5, is a fine, deep, and mellow blue. It is in small pieces of similar form. The outside of each piece is a paler blue than the inside, the colour of which is more bright and lively than that of the finest verditer. A blue compound.

Muriatic, nitric, and sulphuric acids, produce a slight effervescence with this colour. They appear to brighten it, even with long boiling. Oximuriatic acid has no action on it. It differs therefore from ultramarine, which is destroyed by these four acids, as Clement and Desormes observed. Treated with acids,

Ammonia has no action on it.

ammonia,

Exposed to the flame of the blowpipe it grows blackish, and the continued action of the flame converts it into a reddish brown frit. With borax it fuses before the blowpipe into a greenish blue glass. Treated with potash on a stand of platina it produces a greenish frit, which afterward becomes brown, and at length assumes the metallic colour of copper. This frit is partly soluble in water. Muriatic acid poured into this solution produces a copious flocculent precipitate; and the liquor decanted from this precipitate yields another in considerable quantity with oxalate of ammonia. and the blowpipe.

Nitric acid dissolves with effervescence the residuum, which the alkali could not dissolve, and the solution is green. Ammonia produces in this solution a precipitate, which it redissolves when added in excess, and then the solution becomes blue.

This colour then appears to be composed of oxide of copper, lime, and aluminine. It approaches to verditer in the nature of its principles, but differs from it in its chemical properties. It appears to be the result not of a precipitation, Its composition.

tion, but of a commencement of vitrification, or rather to be a true frit.

Of very ancient use.

The process by means of which the ancients obtained this colour appears to be lost to us. All we can learn, on consulting the annals of the arts, is, that the use of this colour dates from ages long prior to the destruction of Pompeii. Mr. Descotils observed a lively, bright, and vitreous blue colour, in some hieroglyphic paintings in Egypt; and he satisfied himself, that this colour was prepared from copper.

Somewhat analogous to blue verditer.

Considering the nature of the constituent principles of this colour, we can compare it only with the verditer of the moderns; but with regard to its use in the arts we may set against it to advantage our ultramarine and smalt, particularly since Mr. Thenard has made known a preparation of the latter, which admits of being used with oil. But verditer has neither the brightness nor permanence of the ancient colour; and both ultramarine and smalt are more costly than a composition, the three ingredients of which are so cheap. It would therefore be worth while, to endeavour to discover the process for manufacturing this blue colour.

A light blue, similar to the preceding.

No. 6 is a light blue sand, mixed with a few whitish particles. Analysis shows in it the same principles as in the preceding colour; and it may be considered as a composition of the same nature, in which the lime and alumine are in larger proportion.

Rose colour.

I have only to examine No. 7. This is a fine rose colour, soft to the touch, reducible between the fingers to an impalpable powder, and giving the skin the colour of a pleasing bloom.

Action of the blowpipe on it,

This colour, exposed to the blowpipe, first blackens, and afterward becomes white. It emits no perceptible smell of ammonia.

and of acids.

Muriatic acid dissolves it with slight effervescence. From this solution ammonia throws down a flocculent precipitate, which is completely redissolved by potash.

Contains no metal.

Neither infusion of galls nor hydrosulphuret of ammonia indicates the presence of any metal in it.

This

This rose colour may be considered as a true lake, in A lake which the colouring principle is mixed with alumine. Its properties, its tint, and the nature of its colouring principle, give it an almost perfect similarity to the madder lake, probably from madder. which I have mentioned in my Treatise on Dyeing Cotton. The preservation of this lake for nineteen centuries without any perceptible alteration is a phenomenon, that must astonish the chemist.

Such is the nature of the seven colours, which have been Used as paints put into my hands by her majesty the empress. They appear to have been absolutely designed for painting: yet, if we examine the glaze or coating of the Roman pottery, and perhaps for pottery. vast quantities of fragments of which are found in all places, where their armies successively established themselves, we shall readily be of opinion, that most of these earthenware may have been employed, to form the coating of this earthenware.

In fact, most of this pottery is covered with a red coat, Roman earthenware, which is in no degree vitreous, and may have been given by the yellow ochre, or the brown red, reduced by trituration to a fine paste, incorporated with some mucilaginous, gummy, or oily substance, and laid on with a pencil. Mr. d'Arcet, who has examined the Roman pottery with great skill, has a vase, the substance of which is of a dull red, and the surface of which was coated with something of this coated, kind. The place where the workman left off coating the vessel may be seen; and on the bottom, which is not coated, may be seen red strokes, made by the workman to try his colour or his pencil.

It is not uncommon to find other vessels, the substance of which is of a different colour from the red coating, that covers the surface.

Perhaps the Romans even employed saline fluxes, to facilitate the baking of the outer coat of their pottery. and perhaps saline fluxes used.

Mr. d'Arcet has perfectly imitated the white covering of White of the Etruscan vases. the Etruscan vases, by using a clay that bakes white, with which he mixed a twentieth part of borax.

It appears, that in the first century of our era the Romans Metallic fluxes unknown to the Romans. were unacquainted with the use of metallic fluxes, to fix and vitrify the coating of pottery. At least the analysis of

the coatings of Etruscan vases, and red, white, and brown earthenware, afforded no indication of metal either to Mr. d'Arcet or me. It was not till a later period, that sulphurets of copper and lead, and oxides of lead, were employed for this purpose. Occasionally indeed we find these metallic coatings on some vases dug up; but I conceive them to have been fabricated subsequently to the time when the Romans possessed Gaul; for all those I have examined, the origin of which evidently dates from the former period, give no trace of lead or copper when analysed.

Black glaze.

Sometimes the black colour alone exhibits marks of vitrification. I have even seen several specimens of ancient pottery, in which this character is indisputable; and I have always thought, that a vitreous lava formed the base of these coatings, the fusion of which, naturally easy, was farther promoted by a mixture of saline fluxes. I published my work on this subject five and twenty years ago; Mr. Fourcroy applied it in a very happy manner in his manufactory at Paris; and Mr. d'Arcet has confirmed my opinions by his own experience.

Their pottery baked with a low heat.

The Roman pottery however, particularly the Etruscan vases, was baked with a very slight heat compared with that we now employ. It may be estimated at 7° or 8° of Wedgwood's pyrometer; and at this degree, as Mr. d'Arcet has shown, we cannot employ the oxides of lead, which then penetrate into the substance, and leave the colour without any gloss on the surface.

Far inferior to us in this manufacture.

No doubt we are far superior to the ancients in the art of pottery. The numerous series of metallic oxides, successively discovered and applied, has furnished us with the means of enriching our pottery with a variety of colours equally brilliant and substantial; at the same time that a better chosen mixture of earths has enabled us, to obtain the greatest degree of hardness with almost absolute infusibility: but the Etruscan vases will always be prized for the beauty, elegance, and regularity of their forms; and I thought, that whatever relates to the history of the arts among the Roman people would be acceptable to those, who interest themselves in the promotion of manufactures.

X.

Remarks on the Introduction of Air into the Blood through the Lungs, in Answer to Mr. ACTON. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

YOUR correspondent, Mr. Acton, appears rather hastily to accuse Mr. Ellis of "a most singular perversion of one of Mr. Bichat's experiments" in the last number of your Journal. It seems to be the object of Mr. E., in the first paragraph alluded to, to show, that when air is forced into the blood, through the lungs, it quickly destroys life; and in support of this position he quotes facts from the writings of Haller, Girtanner, and Bichat, which abundantly establish that point. According to Mr. Acton however, Bichat is said to consider these experiments, as "affording a proof of the passage of the air into the blood, through the lungs, in addition to that of healthy respiration." Does Mr. Ellis deny this? On the contrary, has he not brought forward these experiments expressly to prove it, with the additional circumstance, that it speedily occasions death? Vindication of Mr. Ellis.

But, by "a most singular perversion" of Mr. Ellis's meaning, Mr. A. applies these experiments to another part of that author's work, where he evidently appears to be speaking only of *natural* respiration, and makes no allusion whatever to the *forcible* injection of air into the blood, which fact he had before admitted for a very different purpose. In the language of Mr. A. I dare not say this was intended; but it is "wonderful," if the application be just, that he did not rather undertake to show, that Mr. E., in the two passages quoted, had contradicted himself, than that he had perverted the experiments of Mr. Bichat.

I am, &c.

J. F.

P. S. With respect to the great question, whether a portion of the oxygen, consumed in respiration, be absorbed by the blood in respiration. Oxygen gas not absorbed by the blood in respiration.

the blood as Mr. Acton supposes, or whether it be not entirely converted into carbonic gas as Mr. Ellis maintains, I do not presume to venture a decided opinion: but I must be allowed to say, that, notwithstanding the numerous experiments of Mr. Acton in behalf of the former opinion, the latter has received no inconsiderable support from the recent experiments of Dalton and Thompson*, and Messrs. Allen and Pepys†.

XI.

Letter from Mr. ROBERT BANCKS concerning the Meteorological Journal.

To Mr. NICHOLSON,

SIR,

Height of the thermometer.

IN consequence of the letter of your anonymous correspondent, which you had the goodness to show me agreeably to his permission, I have been endeavouring to discover the cause of my statement of the height of the thermometer not agreeing precisely with those of others, particularly on the hottest day of July 1808.

Situation of the instruments.

The account of the situation of the instruments has been given in your 21st volume, p. 79. I first placed with my thermometers two or three by other makers, the best I could procure: but could find no difference worth notice. When standing near them indeed a little while with a friend, to examine and compare them attentively, we repeatedly found, that the thermometer nearest to which we stood always rose a little the highest; no doubt owing to the heat communicated from our bodies.

The heat not communicated quickly enough to the contiguous air.

From the circumstance however, that the thermometer appeared to give generally too low a temperature for the highest, and too high for the lowest, when they deviated from the Journal of the Royal Society; I was induced to suppose, that the air contiguous to them might be too slow

* Syst. Chem. vol. V, 3d edit.

† Phil. Trans. 1808.

in acquiring the general temperature of the atmosphere; and conceived this might be owing to their being placed in a yard of rather too confined dimensions. I therefore placed other thermometers, by way of comparison, at the height of 17 feet from the ground, in a situation where they were equally protected from reflected heat, but were of course in a less confined part of the atmosphere. My conjecture was in some degree verified; for, on a careful examination for several weeks, I have found the thermometers above apparently a little more sensible of change; though still the difference between them has never been great.

Accordingly I have since registered from the thermometers in this situation; and I am inclined to think, that few can be found superior to it in the advantage of not being affected by any reflected or adventitious heat or cold. From what I have observed it is probable, as I noticed at the time, that my statement of the heat in July 1808 was a little below the truth: but if the difficulty of finding a situation totally unaffected by reflected or communicated heat be considered, I am persuaded that much greater errors in excess were made by other observers, than mine in defect.

Thermometers
now placed
higher.

I am, Sir,

Your humble servant,

R. BANCKS.

SCIENTIFIC NEWS.

Proceedings of the French National Institute.

THE Class of Mathematical and Physical Sciences has proposed the following prize question. French National Institute.

The first inquiries concerning sound date very high in antiquity. The proportions of the length of strings producing different notes are ascribed to Pythagoras: but this branch of science made no remarkable progress before the end of the seventeenth century. Sauveur, a member of the French Academy of Sciences, showed by very ingenious experiments, that the sounding string was divided into seven. Investigation of sound.
Sauveur.

ral waves, separated by nodes, or points of rest; and he determined the absolute number of vibrations that constitute each note, deduced in the first place from delicate and curious experiments, which he compared afterward with the algebraic formulæ derived from the theory of the centres of oscillation; as appears in the *Memoirs of the Academy* for 1713.

Taylor.

Taylor, in his *Methodus Incrementorum*, published in 1717*, treated the problem more profoundly, on the hypothesis, that the forces acting on the material points of the system are proportional to their distances from a right line drawn from one fixed point to the other, so that these points all arrive at the right line at the same time. Twenty or thirty years after Daniel Bernoulli farther developed the theory of Taylor; but for the general and strict solution of the problem we are indebted to d'Alembert and Euler. These great geometricians first employed the differential equation of the motion of the sonorous chord, which is with partial differences, and of the second order. This equation was first found and summed up by d'Alembert, but Euler was more sensible of its generality.

Bernoulli.

D'Alembert & Euler.

Sonorous tubes.

An equation of the same order is applicable to the oscillations of air in tubes; and does not change, when from the case of the simple line we proceed to cases of two or three dimensions.

strings,

In the problems of which we are speaking the order of the differential equation of the motion is connected with the manner, in which we consider the effects of elasticity in the body moved. It has been here applied to a chord stretched between two points. If the chord be let loose at one of these points, being perfectly flexible, it is incapable of producing any acoustic phenomenon.

and springs.

It is otherwise if the chord be a spring properly so called. In this case, confining it if you please to a single fixed point, the spring set to vibrate will produce a perceptible sound, if its oscillations exceed 24 per second: but the differential equation of this movement will be of the 4th order. The first problem may be considered as a particular

* Taylor's paper on the motion of tense strings was published in the *Phil. Trans.* for 1713.

case of the second, abstracting the spring: but the converse does not hold.

The essential difference between the questions of the movement, considered in each of these points of view, in the case of a simple line, leads us immediately to conceive, that we must find differences of the same kind, and in particular a great increase of difficulties, when we introduce two dimensions into the calculation. The acoustic phenomena exhibited by parchment stretched, as on a drum-head, are referrible to those of the chord; the phenomena of metallic plates, to those of the spring.

Euler, in his paper *de Motu vibratorio Tympanorum*, has considered the parchment as composed of threads crossing each other at right angles. A geometrician of the Institute has published in one of its volumes some researches on this subject, contemplating it in the same point of view. The differential equation of the motion, which is partial and of the 2d order, cannot be summed up, at least in finite terms.

In his paper *de Sono Campanarum* Euler attempts to reduce the vibrations of hard surfaces formed by revolution to those of circular elastic rings, of which he considers them as an assemblage, situate in planes perpendicular to the axis of revolution, and supposing the effect of the vibration to be a variation of the lengths of their diameters. He here arrives at an equation with partial differences of the 4th order, not summable in finite terms.

This is all that geometricians have been able to effect with regard to the problems of sonorous bodies considered in the case of two dimensions; and even introducing simplifications, which, it cannot be denied, alter the natural state of things, so that the results of analysis cannot be applicable.

These hypothetical simplifications are particularly inadmissible in respect to vibrating surfaces of metal, or a substance naturally elastic. In the most simple case, that of a plane, it is obvious, that Euler's hypothesis of the vibration of surfaces of revolution is not applicable. We have not even the differential equations of the motion for vibrations of this kind, considering their phenomena as nature presents them; and to find these equations would be an interesting subject of meditation to geometricians, which would contribute

contribute equally to the advancement of natural philosophy and mathematics.

Chladni.

Happily Mr. Chladni has done for the vibrations of elastic surfaces what Sauveur did a century ago for the stretched chord. He has discovered, and rendered perceptible in a very ingenious manner, by the arrangement dry sand takes on vibrating plates, undulations with points of rest interposed. His majesty the emperor and king, who has seen the experiments of Mr. Chladni, struck with the influence that the discovery of a strictly accurate theory, capable of explaining all the phenomena rendered sensible by these experiments, would have on the progress of natural philosophy and mathematical science, has desired the class to make it the subject of a prize, to be proposed to all the learned of Europe. The class accordingly announces it in these terms.

Prize question. "To give the mathematical theory of the vibrations of elastic surfaces, and compare it with experiments."

The prize will be a gold medal of the value of 3000 f. [£125], to be awarded at the public meeting on the fifth monday in january, 1812. No work will be received after the 30th of september, 1811.

Report on Mr.
Chladni's
Theory of
Sound.

The following is an abstract of the report adopted by the class of mathematical and physical sciences, and that of the fine arts, on the 13th of february and 18th of march, 1809, on Mr. Chladni's work concerning the theory of sound.

This treatise, published in German in 1802, and about to be translated into French, contains every thing of importance in his first work, which appeared in 1787, and is enlarged by considerable additions. Under the title of acoustics, it is divided into four parts, which treat, 1, of the numerical ratios of the vibrations of sonorous bodies; 2, of the laws of the phenomena they exhibit; 3, of the laws of the propagation of sound; 4, of the physiological part of acoustics.

Number of vi-
brations in
notes.

The first contains little but what is already known. To determine the absolute number of vibrations in a note however, Mr. Chladni does not employ a chord, but a slip of metal fixed at one extremity, and long enough to allow the oscillations it makes in a given time to be counted. Their
number

number is to that of the vibrations of another slip of metal, taking place at the same time and under the same circumstances, in the inverse ratio of the squares of their lengths.

In this part too Mr. Chladni treats of the temperaments proposed by different persons. He prefers that adopted by Rameau, which renders the 12 semitones included in the octave perfectly equal to each other, by making them answer to 12 geometrical mean terms between the two extremes.

Temperaments.

In the 2d part we find the author's discoveries. He first examines the vibrations of chords and rods, and distinguishes three sorts, the transverse, longitudinal, and those which he calls gyratory. The first take place when a chord or rod is struck in a direction perpendicular to its length. But a rod, that would produce a certain note when thus struck, would emit a very different one, if rubbed with a piece of cloth in the direction of its length. If the rod be of glass, the cloth must be wet; if of any other substance, dry. These vibrations, which he terms longitudinal, he has found subject to the same laws in a solid rod, as the longitudinal vibrations of the air in an organ-pipe; and he has given a table of these vibrations for different substances, such as glass, metal, and wood.

Rods have three different kinds of vibration, producing different notes.

Notes still different from those emitted in the two preceding circumstances are produced, when a rod is rubbed in a direction very oblique to its axis. Mr. Chladni gives the epithet of gyratory to the vibrations resulting from this kind of friction, because he supposes, that the particles of the substance acquire a movement of rotation or oscillation round its longitudinal axis. He says he has found, that in these vibrations the numerical ratios are the same as those of the longitudinal vibrations, but that the tones of each rod are a fifth higher.

Each series of inquiries abovementioned has been made with rods fixed at each end, merely supported at one or both ends, fixed at one end and supported at the other, and loose at each end. Each of these circumstances occasions a difference in the results. Mr. Chladni has likewise examined the vibrations of curved rods, forks, and rings. Euler applied the last species of vibrations to the phenomena of

Experiments made with rods fixed in different ways.

of the sound of bells; but Mr. Chladni has shown very truly, that his hypotheses do not accord with nature.

Vibrations of
plane & curved
elastic sur-
faces.

The last two sections of this part are devoted to the vibrations of plates and bells, or plane and curved surfaces in general, a subject altogether new in experimental philosophy; and which, notwithstanding the striking regularity of the phenomena, has resisted the efforts of the able geometers, who have attempted to treat on it.

Elastic plates.

Mr. Chladni has ascertained the places, which the tones we may draw from plates by giving them different forms, and by causing them to sound in different methods, occupy in the musical scale. But these inquiries are particularly interesting, when combined with those for determining the portions of each plate that have distinct and coexisting vibrations, and the remarkable curves that form their perimeters. For these experiments the plate, covered with fine dry sand, is to be held between the thumb and one finger, the ends of which press on directly opposite points of the two faces, while a bow is drawn over some point of its perimeter. Sometimes a third finger is applied at different points of one of the faces, to vary the results of the experiments. The point of support is always in one of the curves of equilibration. The figure of these curves, and their arrangement, depend on the position of the point of support, that of the point to which the bow is applied, and that of the different sounds we wish to produce by rubbing the bow in different ways on the same point. A change in either of these produces a correspondent change in the curves.

Examination
of these by
Paradisi.

While speaking of these curious phenomena, we cannot avoid noticing a paper inserted in the first volume of the Transactions of the Italian Institute, entitled Inquiries concerning the vibrations of elastic plates. The author, Mr. Paradisi, says in a note, that he was led to make his experiments by a passage in the *Bibliothèque Britannique* where Mr. Chladni's were described. Having provided an apparatus, by means of which he could keep the plates fixed at any point of their surfaces without the assistance of the fingers, he first perceived, that the curves of equilibration did not arrive at settled figures, till after a gradual and continual succession of variable figures; the generation of which, being

The vibrations
go through a
series of
changes.

ing examined by him with great attention, led him to new inferences respecting the theory of these curves.

Thus for example, if we take a rectangular parallelogram of glass 9 inches long and 3 broad, fix it in the line of its longer axis one sixth of its length from the end, and apply the bow to one of the longer sides of the parallelogram at one third of its length; the lines in the sand, when come to a state of rest, will divide the surface into eight equal rectangles by a right line in the direction of the great axis, and three equidistant right lines parallel to the shorter sides. But Mr. Paradisi found, that on causing the plate to vibrate by a succession of very little touches with the bow, 8 semicircles were first obtained, the centres and diameters of which were placed symmetrically on the longer sides of the parallelogram, and the point of application of the bow was one of these centres. These semicircles gradually increase: those on the same side from separate become tangents, and afterward penetrate into each other, leaving between them rectilinear lines perpendicular to the longer sides; and in proportion as these lines increase in length, the arcs flatten as they approach the greater axis of the parallelogram, with which they are at length confounded.

In other experiments Mr. Paradisi obtained whole initial circles formed on the surface of the plate, and semicircles with their diameters resting both on the longer and shorter sides of the parallelogram. The velocity of the grains of sand placed in the perimeters diminished in proportion as the radii increased.

Mr. Paradisi applies the term of *centre of vibration* to the centre of the circle that forms round the point to which the bow is applied, and that of *secondary centres* to those of the other circles. Supposing afterward, that when the system of curves is arrived at a fixed state, any given element of these curves is directed by the result of several forces, the actions of which emanate from these different centres of vibration, and are functions of their distances from the element of the curve in question, he arrives at a differential equation between the coordinates of this element, the summation of which would require the form of the functions, that represent the laws of the actions of the forces, to be known

Examples.

Centres of vibration and secondary centres.

known. He promises us farther inquiries on this subject in another paper.

We must refer to the memoir itself for his other experiments, among which are some interesting ones on changes in the fixed point, and in the point to which the bow is applied, without producing any in the figure or arrangement of the curves.

Vibrations of
bells.

Mr. Chladni concludes his second part with reflections on the vibrations of bells, and of curved surfaces in general, and on the coexistence of vibrations in sonorous bodies. He speaks of the theory and hypothesis of Euler respecting the sound of bells; of Rameau's system of the fundamental base; of the musical system of Tartini, founded on experiments, which, according to Mr. Chladni, were known in Germany long before Tartini made use of them, and which may be considered as the inverse of Rameau's; and lastly of the combination, which takes place in certain circumstances, of the vibratory with other kinds of motion.

Propagation of
sound through
different sub-
stances.

In the third part the author first considers the propagation of sound as effected by the air and different aeriform fluids: he then examines the cases where it takes place through the intervention of liquid and solid substances. We here find the experiments, which the author made in concert with Prof. Jacquin of Vienna, on the vibrations of various kinds of gas; and conjectures on the cause of the difference between the observed velocity of the propagation of sound through air, &c., and that given by theory.

The committee conceive, that the two classes ought to bestow distinguished encomiums on the discoveries of Mr. Chladni respecting the philosophy of sound; and that it is an object of importance, to direct the attention and emulation of the learned to those physico-mathematical researches, to which his discoveries may give rise.

Signed, de Lacépède, Haüy, Méhul, Gossec, Gretry, Le Breton, de Prony.

Imperial

Imperial Academy at Petersburg.

The following prize subject is proposed by this academy Imperial Russian Academy.
for the year 1810.

“To improve the theory of sluices, and thence to deduce Prize question for 1810,
rules for constructing these important works in the most advantageous manner; so that they may be used with all possible security and speed, be attended with as little expense as may be for their construction and keeping in repair, and incur no waste of the water required for the passage of loaded vessels more than is absolutely necessary.”

And for 1811. “To give a complete comparative chronology, and, if possible, corrected and verified, of the Byzantine authors, from the foundation of the city of Constantinople till its conquest by the Turks.” and for 1811.

The prize for each is 100 Holland ducats [£46 5s.], and the answers must be sent before the 1st of July in each year.

Mr. Peter Alemani, of Milan, has analysed a new species of urinary calculus. In 100 parts he found pure magnesia 51, silex 20, phosphate of iron 11.84, carbonate of magnesia 4. The volatile substances and loss amounted to 3.16. [One of these numbers has evidently a deficiency of 10.] Analysis of a urinary stone.

Dr. G. Melandri, of the same place, is examining the artificial tannin of Mr. Hatchet, but in another point of view. His researches are on the tannin of different plants and vegetable products. He thinks, that it is not an oxide of carbon: but an oxide with a binary, or more probably ternary radical. The nitrogen of the nitric acid must enter into its composition; as must the nitrogen of the animal charcoal, since this succeeds better than vegetable charcoal. He believes too, that hydrogen enters into it, though in small quantity. Artificial tannin.

On analysing deadly nightshade, *atropa belladonna*, he discovered in the leaves a salt never before observed in vegetables, the oxalate of magnesia, joined with free oxalic acid. The other substances in them were oxalate of lime, Analysis of deadly nightshade.
muriate

Sensible test
of acids and
alkalis.

muriate of potash, a soft green resin, an animal extract, mucilage, and oxigenizable extract. In the berries he found as sensible a test of acids and alkalis as the infusion of mallow flowers. By pouring alcohol on the expressed juice of the ripe berry, the purple fluid will be coagulated by the precipitation of the mucilage. This coagulum is to be well washed with the same alcohol, and the tincture filtered off. If this tincture be diluted with water till it has no longer any perceptible colour, it will become green with alkalis and red with acids. The purplish colour of this tincture changes to a yellow in time, but it still retains its property of detecting the smallest portion of acid or alkali in water.

Potassium ob-
tained in vari-
ous ways.

Mr. Ritter has obtained the metallic product of potash with almost all the metallic substances yet known, when they are employed as the extremity of the negative conductor, and always fine and perfect. Arsenic alone produces it of a shining black or blackish colour. He has obtained it also by employing charcoal and plumbago as conductors: but not with the gray crystallized oxide of manganese, which is merely deprived of its oxygen in the process. When tellurium was placed in potash as the extremity of the negative wire, it did not produce bright metal of potash, but a brown dirty substance. Mr. Ritter then

Tellurium dif-
fers in some
respects from
other metals.

took tellurium for a negative wire, and immersed it in pure water in which was likewise the positive wire, and immediately streaks of a brown black were produced, which, separating from the tellurium, fell to the bottom of the water, and from the manner in which they were produced, and the place of their origin, they must have been hidruret of tellurium. Thus tellurium produced no metal of potash because it absorbs all the hydrogen itself. The button of tellurium, purified afresh, was employed as a positive wire in pure water; and, what must excite more astonishment, it remained brilliant, formed no oxide, and gave out a great deal of gas. Thus of eighteen metals subjected to Mr. Ritter's experiments, tellurium is the only one, that produced a hidruret at the negative pole; and the fourth, that with gold, platina, and palladium, gives out gas at the positive pole. Does tellurium then commence a new series

of

of metals, which comport themselves with respect to the hydrogen of water as others toward the oxygen of this fluid?

Dr. Seebeck, of Jena, has obtained indications of an amalgama with magnesia and alumine. Magnesia and alumine perhaps metallic. Artificial succinic acid.

Mr. Trommsdorff has prepared an artificial succinic acid. For this purpose he employs the saccholactic acid of Scheele, which he introduces into a retort and subjects to dry distillation. The products of this distillation deserve farther inquiry.

He has likewise examined the sulphuretted alcohol of Lampadius, and found in it several new properties. As it contains no carbon, he thinks it may be called oleous hydro-guretted sulphuret. It readily dissolves phosphorus, and in large quantity; one part dissolving eight of phosphorus and still remaining liquid. This solution of phosphorus readily takes fire in the open air. In close vessels it may be decomposed by heat. The sulphuretted alcohol first passes over, though not quite free from phosphorus. Sulphuretted alcohol of Lampadius contains no carbon.

Fecula dissolved in boiling water undergoes a remarkable change, when evaporated over a moderate fire. It becomes a semitransparent horny mass perfectly insoluble in hot water. Wetted, and kept five months in a pretty warm place, Mr. Trommsdorff could not find it exhibit any signs of fermentation. Fecula changeable by heat.

Mr. Trommsdorff repeated Mr. Cadet's experiments on the solution of camphor in distilled water*. He found them accurate; but he also found, that the camphorated water is rendered turbid by pure soda, and consequently will not serve as a test to distinguish this from potash. Mr. Vogel has made some trials, that confirm this: but soda combined with a certain portion of carbonic acid does not precipitate the camphor. Camphorated water not a test of soda.

* See Journal, vol. XIX, p. 26.

METEOROLOGICAL.

METEOROLOGICAL JOURNAL,

For NOVEMBER, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker,
in the STRAND, LONDON.

OCT. Day of	BAROME- TER, 9 A. M.	THERMOMETER.				WEATHER.	
		9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	Day.	Night.
27	30.28	50°	50°	52°	47°	Fog	Foggy
28	30.28	48.5	50.5	53	46	Ditto	Heavy fog
29	30.26	47	49	51	46	Ditto	Rain
30	30.22	48	49.5	52	46	Ditto	Cloudy
31	30.11	48	46.5	50	41	Fair	Foggy*
NOV. 1	30.10	49	44.5	52	42	Cloudy	Rain
2	30.20	45.5	47	49.5	39	Fair	Cloudy
3	30.14	44	45	47.5	40	Rain	Rain
4	29.93	44.5	45	46	40	Ditto	Ditto
5	29.84	42.5	42	45	37	Ditto	Cloudy†
6	29.92	41.5	43	46	37	Ditto	Rain
7	30.06	42	45	47	40	Cloudy	Fair
8	30.39	42.5	43	46	40	Fair	Ditto
9	30.42	43	46.5	47.5	40	Cloudy	Cloudy
10	30.29	47	47.5	50.5	43	Ditto	Ditto
11	30.15	44.5	43	50	49	Ditto	Ditto
12	29.91	44	44	46	41	Ditto	Ditto
13	29.74	43.5	44	46	36	Ditto	Ditto
14	29.72	38	46.5	47.5	34	Fair	Rain
15	29.70	36	35	39.5	29	Ditto	Fair
16	29.71	31.5	34	37	32	Ditto	Ditto§
17	29.48	39	36.5	45	30	Rain	Ditto
18	29.73	34	36	38	30	Snow	Ditto
19	30.23	33	30	37.5	26	Fair	Ditto
20	30.43	28.5	34.5	37	36	Ditto	Ditto
21	30.28	38	35.5	41	36	Ditto	Ditto
22	30.16	39	45.5	49.5	41	Ditto	Ditto
23	30.03	43.5	47	49	40	Rain	Rain
24	29.39	42	43	47	39	Ditto	Cloudy
25	29.78	38	40	42	37	Fair	Ditto
26	29.10	42	38	43.5	32	Ditto	Fair

* At 6 P. M. stars visible; at 9, heavy fog; at 11, starlight.

† At 11, starlight and clear.

‡ Commencing rain at 10 P. M.

§ Snow in the night, the morning milder.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO VOL. XXIV.

ARTICLE I.

Memoir on the Triple Sulphuret of Lead, Copper, and Antimony, or Endellion. By M. LE COMTE DE BOURNON, F. R. and L. S.

(Concluded from Page 260.)

PART II.

Observations on Endellion, as being the Result of a triple Combination, and on the different Sulphurets of Copper.

THE Royal Society has printed in the first part of the Philosophical Transactions for 1808 a paper*, in which the constituent principles of endellion, as well as the manner in which they combine, are discussed †. The author

Paper on endellion in the Phil. Trans.

* See Journal, vol. xx, p. 332.

† *Additional note.* I mean Mr. Smithson's paper already mentioned. The Royal Society having printed a critique on the crystallographical part of my first memoir on endellion, however I might feel hurt by the style of that critique, I thought it better not to notice it, than to expose the transactions of that illustrious and respectable body to be made the scene of a dispute, which certainly could not be more misplaced. I therefore presented my new memoir on endellion to the Royal Society, merely as the result of continued and extended

Doubts of the existence of higher than binary combinations.

Ultimate particles of bodies have a regular figure.

Combinations more than binary may exist.

author there professes his doubts of the existence of triple, quadruple, and greater combinations; and his opinion, that all combinations are binary. In consequence he endeavours to refer to one of the latter the nature of the compound, that gives rise to endellion; considering it as formed by the intimate combination of sulphuret of lead, or galena, and that kind of copper ore, which the Germans call fahlertz. I cannot conceive on what reasons the author grounds his opinion, that there can be no triple, quadruple, or greater combination. On the contrary the possibility of these combinations seems to me demonstrated by the simple facts, that I have already brought forward in the second volume of my mineralogy, p. 390, in order to show, that the molecules of bodies, considered as principles of minerals, possess, as well as the integrant molecules which result from the combination of these, the property of having a regular figure. The act of combination of these molecules, in

observations, which had enabled me to make this substance, which is peculiar to England, more thoroughly known, and to render my account of it more complete.

The second part of my paper was intended to include some reflections on a fact highly interesting both to the mineralogist and the chemist, which is the possibility or impossibility of the existence of triple, quadruple, and other combinations in the mineral kingdom. Mr. Smithson, in one part of his paper, sought to establish the principle, that all combinations could only be binary, and adduced endellion in confirmation of his opinion. After having laid down the reasons, that seem to me to preclude all doubt of the possibility of more than binary combinations, it was necessary for me to show the weakness of the argument deduced from endellion; which could not be made to answer this purpose without giving an arbitrary proportion of the component parts of the two sulphurets, the binary combination of which produced it, or at least a proportion different from that usually admitted by chemists. I confess, however, that, had I not found occasion to answer the critique included in the same paper, it would not probably have been in the Philosophical Transactions, that I should have pointed out this obvious error. However, if the committee of the Royal Society had requested me, to suppress this part of my paper, I should not have hesitated a single moment to comply with its wishes, however interesting I conceived it to be.

forming the integrant molecule, which is the immediate result of this combination, differs then in no respect from that, which afterward unites the integrant molecules. Now it is very easy to conceive, and even to adduce a number of instances of the formation of a crystal of any determinate figure (representing the integrant molecule of a compound substance) that shall be composed of the intimate union of three, four, or even more crystals of different forms, which in this case would represent the molecules of the substances that compose it; and which would enter into the composition of the crystal in equal numbers, or, which is more commonly the case, in unequal numbers. It is indeed to the property, Particles unite to form secondary particles of a different figure in chemical combinations. which the molecules of minerals have, of uniting intimately with each other, so as to produce a new molecule of a determinate figure, that I attribute in part the formation of those minerals, which are commonly said to be the effect of chemical combination. Every combination of the substances of which a mineral is composed seems to me to require, that the form of the molecules of each shall bear such a relation to those of the rest, that their faces may respectively coincide, so as to produce collectively a molecule, the form of which shall be at once determinate and invariable. It is this relation between the several component molecules, which in all probability determines their action upon each other, known by the name of "attraction of composition;" or which is at least a principal cause of this. Upon the same principle we may account for the proportion of the several substances, which must necessarily vary, according to the number of these molecules, the forms of which are different, and the mode in which they arrange themselves, so as to produce a new molecule, the form of which shall be determinate. When the molecules are wholly dissimilar, and there is no relation whatever between their faces, it is not possible for them to combine, so as to generate a new substance properly so called and capable of crystallization.

The real existence of these triple and greater combinations is farther demonstrated by facts. We know, for instance, that there exists a substance which differs from gypsum, or the combination of lime with sulphuric acid and Gypsum the result of a triple combination.

water, only in not containing of the last of these three constituent principles. Yet this simple privation, by producing a substance of a different form, harder, heavier, and possessing different chemical properties likewise, shows, that water, which combines as a principle with lime and sulphuric acid in the formation of gypsum, is necessary to its formation, and that gypsum is consequently the result of a real triple combination.

Objection.

It may be objected indeed, that in gypsum there exists only a double combination between the molecules of sulphate of lime on the one hand, and those of water on the other. But, if this were the case, when the water had been expelled from the gypsum by calcination, the sulphuric acid being left, there should still be an intimate combination between the molecules of the sulphuric acid and those of the lime: and the plaster, which results from this calcination, should exhibit a substance precisely of the same nature as that known by the name of anhydrous gypsum, or bardiglion (as I call it,) whereas in fact there is no similitude whatever. The bardiglion reduced to powder, either before or after calcination, possesses none of the properties of gypsum, and does not absorb any water whatever, so as to combine with it, and thus acquire the solid form. By the great avidity with which calcined gypsum seizes on water, the moment it comes into contact with it, we discover that calcination, by taking from each of the integrant molecules of gypsum (composed of those of sulphuric acid, lime, and water) the molecules of water, has changed the integrant molecules of this substance into new molecules, consisting simply of those of lime and acid, but having only an incomplete form: or, if I may be allowed so to express myself, we perceive that this calcination has carried away a part of each of the integrant molecules of gypsum, and left in each one or more cavities, the sides of which, having a very powerful affinity for the corresponding sides of the molecules of water, seize on them as soon as they have an opportunity of so doing, and fix them again in the places to which they belong. Gypsum is then in fact the result of a triple combination.

Answered.

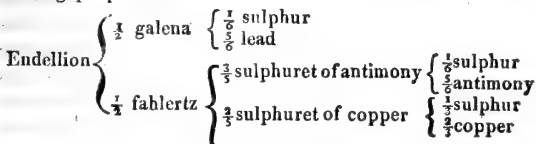
But

But is it certain, that the integrant molecule of endellion is simply the result of a triple combination of the integrant molecules of the three sulphurets of lead, copper, and antimony? and is it not more natural to consider it as the result of a quadruple combination of the molecules of sulphur, lead, copper, and antimony? I confess, I am much inclined to consider endellion in the latter point of view, particularly when I observe, that the sulphur, which from the proportion in which it exists in the three sulphurets should amount to 20.03, forms only 17 hundredths of this substance.

In referring endellion to the binary combination of galena and fahlertz, the paper above quoted gives the following proportions.

Endellion more probably a quaternary than a ternary combination.

Composition of endellion, as given by Mr. Smithson.



From these proportions it would follow, that the sulphuret of lead contains $83\frac{1}{2}$ of lead, and $16\frac{2}{3}$ of sulphur: that of antimony $83\frac{1}{3}$ of antimony, and $16\frac{2}{3}$ of sulphur: and that of copper $66\frac{2}{3}$ of copper, and $33\frac{1}{3}$ of sulphur.

The proportion in which the sulphur is said to combine with the lead in galena, or the sulphuret of this metal, is the same as is given by Mr. Kirwan. As to the two sulphurets of antimony and copper, the proportions between the sulphur and the metal result probably from the author's own observations, which it is much to be regretted that he has not given.

Those of the sulphurets of antimony and copper differ from other authors.

According to Proust and Bergman, the only two authors, as far as I know, that have established the proportions of sulphur and antimony in the sulphuret of this metal, the sulphur is to the antimony in the proportion of 26 to 74; and this is the proportion hitherto followed by all writers.

Proportions in sulphuret of antimony.

They have in like manner followed the proportion affixed by Klaproth to the sulphur and metal in the sulphuret of copper, which is that of 18.5 to 78.5: and with respect to this sulphuret I will add, that, about the time when I laid before

Proportions in sulphuret of copper.

Analysed by
Mr. Chenevix.

before the Royal Society my first paper on endellion, being desirous of obtaining some additional data with respect to the proportion in which sulphur enters into the sulphuret of copper, I requested the favour of Mr. Chenevix to assist me in my researches, by analysing different varieties of the sulphuret of this metal, with specimens of which I furnished him. This able chemist found in one perfectly pure Cornish specimen, which was regularly crystallized, 19 of sulphur and 81 of copper; a proportion exactly similar to that given by Klaproth, if we consider that the sulphuret of copper analysed by him contained 2.25 of iron, and 0.75 of silice, which did not exist in that analysed by Mr. Chenevix.

Other specimens analysed
by him.

This gentleman at the same time, at my request, took the trouble to analyse seven other varieties of sulphuret of copper; I having supplied him with the specimens, duplicates of which I preserved. One of them only was from Bohemia, and the rest from Cornwall. All of them contained from 0.03 to 0.08 of iron, but no other extraneous substance. They all gave similar results as to the proportions of sulphur and copper, except that the quantity of sulphur was a little greater where the quantity of iron was greater.

Mr. Smithson's
proportions erroneous.

Thus it appears to me incontrovertible, that the proportion of sulphur, admitted in the paper in question as essential to the composition of sulphuret of copper, is much too great; and that the proportion of sulphur, there said to enter into the composition of sulphuret of antimony, is too small: which would totally overturn the proportions, by which the author of that paper endeavours to prove endellion to be produced by a binary combination between sulphuret of lead, and the gray sulphuret of copper named fahlertz. It is possible, for this sometimes happens in metallic sulphurets, that Messrs. Klaproth and Chenevix, in the specimens they analysed, may accidentally have met with varieties of sulphuret of copper, in which sulphur was superabundant, or simply interposed; and in this case their analyses will give too large a proportion: but that two chemists, so eminent for their talents as the gentlemen just mentioned, should constantly find the proportion of sulphur in this sulphuret much less than is assigned in that paper,

per, agreeing at the same time with respect to the proportion in which it enters into this compound, is I believe as complete a demonstration, as chemistry can furnish: at least if there be any error in it, the error must be proved.

The majority of ores in the sulphuretted state, of which copper constitutes a part, being of a gray colour; these ores being very numerous; in some of them the copper being merely interposed; and in the greater number of those of which it is a component part, being commonly intermingled with different metals, most of them sulphuretted likewise: nothing is more difficult, than to distinguish these ores, so as to refer each to the principal and particular type, to which it belongs.

Ores of copper easily confounded.

Among the different species, to which these ores may be referred, it has uniformly appeared to me, that what the Germans call fahlertz belongs to the gray species that crystallizes in regular tetraedra. But this species, in which the copper in the sulphuretted state is in pretty large quantity, is at the same time one of those most subject to admit foreign substances by the interposition or juxtaposition of their molecules. As I have already said in my first paper on endellion, presented to the Royal Society, it appears to me unquestionable, that the essential component parts of the gray tetraedral sulphuret of copper are copper, iron, and sulphur; and the analysis, which I have there said was made by Mr. Chenevix of a variety from Cornwall in well defined crystals, in which he found nothing but these, in the proportions of copper 0.52, iron 0.33, and sulphur 0.14, seems sufficient to prove that these substances are thus proportioned in this ore.

Very liable to extraneous mixture.

The author of the paper, when he gives fahlertz as the second component part of endellion, considered as the product of a binary combination, says that it is composed of $23\frac{1}{4}$ sulphur, 50 antimony, and $26\frac{2}{3}$ copper; a composition that would constitute a variety among the antimonial ores, and has nothing to do with that I have just given. But as it differs materially from all the very numerous analyses, that have been made of different varieties of gray copper containing antimony, it must be the result of the author's own observations, and a particular series of experiments, which

Mr. Smithson's fahlertz a new variety among the antimonial ores.

which it would have been of great importance to make known.

Examination of
fahlertz.

The attention which the different sulphurets of copper appear to me to deserve, as opinions respecting them are not yet settled, induces me to add to what I have said on the fahlertz, or tetraedral sulphuret of copper and iron, the following reflections. Part of them have already appeared in my first paper on endellion; but the observations which I have subsequently had an opportunity of making on the sulphuret of copper enable me to present these reflections to the Royal Society again on a larger scale, and in a manner better adapted to illustrate this interesting subject, on which so much uncertainty prevails.

Extraneous
substances mix-
ed with it.

The substances foreign to fahlertz, or tetraedral gray copper, which observation has hitherto shown to be interposed in it, are silver, lead, antimony, and arsenic; and very frequently these substances appear to be sulphuretted, as well as the copper. An analysis by Klaproth of a variety from Kapnick, in Hungary, has even indicated 0.06 of zinc; and another of a variety from Poratsh, in Upper Hungary, 0.0625 of mercury. These substances, several of which are sometimes found in the variety, that possesses the property of crystallizing, are no obstacle to crystallization: but there are many other varieties, that appear to be destitute of this property. In spite of all the researches I have been able to make on this subject, I cannot establish in a satisfactory manner the characters, that might serve to make them known: in general the gray colour of the crystallizable variety is I think less deep, and its lustre more brilliant, but to this there are many exceptions.

All the gray sul-
phurets of cop-
per liable to fo-
reign mixture.

The fahlertz is not the only sulphuret of copper, that is subject to this interposition of foreign substances; it is the same with all the gray sulphurets of this metal. The various mixtures they contain has even occasioned different names to be given them, and different opinions to be entertained respecting their nature. From the silver, which some of its varieties contain, fahlertz had long the name of *gray silver ore*. Afterward, when it was found, that a great number of other varieties were totally devoid of silver, it was called *gray copper ore*. An analysis, which Klaproth made of a variety

variety from Andreasberg in the Hartz, having afforded him 0·34 of lead, occasioned its removal from the copper to the lead ores; among which many German mineralogists continue to class it, expressing doubts however, respecting the nature of its real component parts. Nothing is more variable than the analyses, that have been made of different gray sulphuretted copper ores, taken even among those that are crystallized; and it is absolutely impossible to found upon these any determinate classification of the species and varieties of this ore, unless we previously establish certain fixed points, to which we may refer them.

In this state of things I conceive we ought to consider the sulphuretted copper ores in two different points of view; the first regarding those that admit a determinate form; the second, such as have not hitherto appeared to admit any, as the weissgueltigertz and graugueltigertz of the Germans, &c.

Division of t
sulphuretted
copper ores.

Among such of these ores as take a determinate form we ought, I think, to consider as belonging to one species those, that constantly take the same primitive form, or some one of its modifications. At the head of this we should place, first, the simple sulphuret of copper, composed of 0·81 copper, and 0·19 sulphur. Its primitive form is a right hexaedral prism, the terminal faces of which are regular hexagons*, and the specific gravity of which is

Distinction of
the species.

Simple sulphu
ret of copper.

5.643.

* My intention is soon to lay before the 'Royal Society a more complete detail, relating directly to these copper ores, in which the form and dimensions of these crystals will be established.

Addition. When I entered into this engagement, I was not aware of the fate, that awaited the paper in which it was inserted.

The height of the regular hexaedral prism, forming the primitive crystal of the simple sulphuret of copper, is to the apothema of the regular hexagon, that serves as its base, in the ratio of 2 to 3, a ratio determined from three different substitutions for the edges of the same hexagons, the planes of which make with the terminal faces for the first an angle of $146^{\circ} 19'$, for the second $138^{\circ} 22'$, and for the third $116^{\circ} 32'$. Frequently all the planes owing to these three modifications terminate the same prism. Often too they reach each others limits, and then give rise to as many hexaedral pyramids, either terminating

Primitive crys
tal of sulphure
of copper.

§ The apothema is a perpendicular line let fall from the centre of the circle in which the hexagon is inscribed, and bisecting any one of its sides.

5.643*. This species, when it is perfectly pure, and without any mixture of iron, may be cut nearly with as much ease as lead, of which it has almost the colour. It cuts perfectly smooth, and with a metallic lustre. This sulphuret alters spontaneously to a deep black by the oxidation of its surface.

Variegated copper ore.

2d, The double combination of copper and iron with sulphur, known by the name of *bunt kupferertz*, which was given it by the Germans, and the composition of which appears to be from 0.60 to 0.65 copper, 0.18 to 0.15 iron, and 0.22 to 0.25 sulphur; proportions deduced from the analyses of ten different specimens, made by Mr. Chenevix at my request. Its primitive form is a cube†; and its specific gravity

terminating in a point, or very nearly so. Frequently these pyramids touch each other at their bases, when, the prism disappearing, they give rise to three different dodecaedra with isosceles triangular faces. In one of these dodecaedra, the planes meet at the summit at an angle of $112^{\circ} 38'$; in another at one of $96^{\circ} 52'$; and in the third at one of $53^{\circ} 8'$. These crystals are almost peculiar to Cornwall; every where else the crystals of sulphuret of copper are very rare.

* This specific gravity was given by two very perfect crystals united together, weighing about 258 grains, and perfectly pure. Authors that have mentioned this substance give its specific gravity from 4.810 to 5.338. Beside that the specific gravity of no substance can vary to such a degree, the greatest is certainly below the truth; it was probably taken from some of its amorphous varieties; and a great number of trials with these has taught me, that their specific gravity varies considerably; and never equals that of the crystals. No doubt this is owing to cavities in their interior, which in fact may be frequently seen on breaking pieces of this sulphuret. In weighing this sulphuret of copper too, we should not take crystals that are grown black or oxidized on their surface; the black oxide of copper not being easily permeable to water, there always remains in this case a great deal of air between the surface of the piece of sulphuret and the water in which it is weighed; and as we cannot entirely free it from this, the specific gravity obtained is always much less than it ought to be.

Crystals rare.

† *Additional note.* The crystals of *bunt kupferertz* are very rare. Cornwall, which has furnished mineralogists with so many scarce species of this metal, has produced some very fine groupings, though but few. The cube, which is the primitive crystal of this substance, is the form

gravity is 5.033. It is not so easily cut as the preceding; and the cut, though smooth, has not the same lustre. Its most common colour has the redness of nickel, which is deeper where cut; but if this sulphuret be ever so little decomposed, it acquires a blueish tint, and afterward assumes the finest colours.

3d, The double combination of copper and iron with Fahlert^z. sulphur, but with a larger proportion of iron than the preceding species. It is the fahlertz, or gray sulphuret of copper and iron; the composition of which is copper 0.52, iron 0.33, and sulphur 0.14; and the specific gravity 4.558. This species is much harder than the preceding; but its hardness varies according to the nature of the different substances frequently interposed in it. It may be scratched, but not cut; and the place scratched has neither the smoothness nor the lustre of the two foregoing species. Its powder varies from a full black to a black with a reddish cast, or a reddish brown. The latter colour, as far as my observations go, always indicates the presence of silver, which is commonly in the state of red antimoniated silver. It is much

form its crystals most commonly assume. Sometimes the places of Chiefly cubical its solid angles are occupied by equal sided triangular planes. Very commonly these cubes have their faces slightly curvilinear. At other times they are merely an aggregation, frequently irregular, of other small cubes, which renders their figure very difficult to discriminate.

To the bunt kupferertz no doubt should be referred the cube, which Werner, Estner, and several other German mineralogists, give as one of the forms of the simple sulphuret of copper, to which it appears to me incapable of belonging. As to the octaedron, given likewise by the same authors to the simple sulphuret of copper, to which it is equally far from belonging, I presume, that some octaedra of red oxidized copper, oxygenized to a maximum at the surface, and turned black to a less or greater depth, may easily have led to the mistake. Formerly there occurred in Cornwall a Variety. variety of bunt kupferertz in thin laminæ superimposed on one another, and frequently of a fine blue colour at their surface. This contained iron in smaller proportion than the bunt kupferertz, but sufficient to render the sulphuret of copper incapable of being cut with the knife, and when cut exhibiting the metallic lustre as the simple sulphuret of copper. The fracture presents a coppery red colour.

more

more liable to decomposition than either of the preceding, particularly when crystallized.

Copper pyrites. 4th, The double combination of copper and iron with sulphur, which is shining and of a deep yellow colour. We have no analysis of this species, except that of Lam-padius, who gives for its component parts 41 copper, 17 iron, and 45 sulphur: but it is very probable, that the specimen analysed by him contained a superabundance of sulphur interposed in its substance; and besides, the proportion of iron given by this analysis is certainly too small*.

From several assays of this copper ore made with Mr. Chenevix, it always appeared to us, that it differed very little, either in its component parts, or in their proportions to each other, from the gray species, which I have said should bear the name of gray sulphuret of copper and iron. The form of its primitive crystal is a regular tetraedron, modifications of which it sometimes admits, though much fewer than the gray sulphuret of copper and iron; and among which we chiefly find the regular octaedron, and the dodecaedron with rhombic faces; but the latter variety, which occurs in Cornwall, is very rare. The specific gravity of this sulphuret is 4.058 †. It is not so hard as the fahlertz. Its fracture is very brilliant, ragged, and as if it were composed of small laminæ intersecting each other in various directions. In decomposition it assumes the most

* *Additional note.* I have lately seen in the Journal des Mines, No. 122, that Mr. Gueniveau, engineer of mines in France, has analysed two varieties of yellow sulphuret of copper and iron: One, from St. Bel near Lyons, afforded him metallic copper 30, metallic iron 23, and sulphur 36. The other, from Baigorry, yielded metallic copper 27.5, metallic iron 29.5, sulphur 31.5 §.

† This specific gravity is a mean of those of four tetraedral crystals, either perfect, or with their solid angles truncated. Authors have hitherto carried this specific gravity to 4.315: but I presume, that it was not taken from crystals, and that the pieces weighed were mingled with sulphuret of iron, which frequently happens. I have found yellow sulphurets of copper and iron, thus mingled, weighing as high as 4.6.

§ See Journal, vol. xxi, p. 148.

lively colours, till at last it loses great part of the copper it contained; and which very frequently in this case, combining with carbonic acid, passes to the state of green copper, leaving a residuum of oxide of iron, which however is still sometimes pretty rich in copper, and is then known by the name of hepatic copper ore.

Care must be taken not to confound this double sulphuret of copper and iron, as is frequently done, with the martial pyrites that contains copper intermingled with its substance, commonly in small quantity, though it is sometimes pretty rich in this metal. From this the double sulphuret is totally different. The form of the martial pyrites containing copper is either a cube, and this commonly striated, or a regular octaedron. The martial pyrites is likewise much harder than the yellow sulphuret of copper and iron, and it is heavier, its mean specific gravity being 4.944*. It must appear very strange, that this sulphuret, having great analogy in its component parts, as well as in its form, with the gray sulphuret, or fahlertz, should have a colour so very different from it, as well as from all the other sulphurets of copper. Endeavouring to account for this, I have always been led to think, that this difference of colour might arise from the iron's being in the perfectly metallic state in the yellow sulphuret of copper and iron, as it is in the martial pyrites, while in the gray sulphuret it is oxidised. This opinion however I only mention as a great probability †.

This not to be confounded with the iron pyrites in which copper is intermixed.

Cause of the difference of colour.

In

* This specific gravity is a mean of those taken from crystals all of different forms. Authors give for it from 4.100 to 4.749. Certainly however they have not taken it in the same manner from crystals, but from amorphous masses; or at least it must have been from very impure crystals, otherwise they would not have varied from 4.1 to more than 4.7; and it would even have been found superior to this maximum.

† I have observed with the greatest satisfaction, that the opinion I had long embraced respecting the cause of the difference of colour between the yellow sulphuret of copper and iron and the gray, and which was inserted in my first paper on endellion presented to the Royal Society, has been verified by the analyses made by Mr. Gueniveau of two varieties of yellow sulphuret of copper and iron from

Another species.

In this case perhaps it would be necessary to make a 5th species among the sulphurets of copper of an ore, which was formerly very plentiful in Cornwall, but is now become rather scarce, and which probably differs from the preceding species by a more or less considerable degree of oxidation in the iron. This ore is of a dull yellow colour, inclining a little green. Its fracture is smooth and dull, and sometimes a little conchoidal. Its grain is extremely fine, and frequently even imperceptible to the eye. Its texture consists of parallel layers, very thin, and distinguishable only by the assistance of a lens, but easily separated by a stroke of the hammer. This ore has never exhibited to me any crystalline form; but it is frequently mamillary, much like the martial hematites. Its surface is commonly smooth, a good deal like that of a metal which has lost its polish. Its specific gravity is 4.157: consequently a little greater than that of the preceding yellow sulphuret of copper and iron. Its hardness is nearly the same. If scratched with a knife, the part scratched appears smooth, and acquires a metallic lustre. On decomposition the surface frequently assumes various colours, but less lively and brilliant than those of the lamellar yellow sulphuret. At other times its surface grows black from the oxidation of the copper, having a good deal the look of an antique bronze, and the more so as it is often partially covered with malachite. This species is frequently mixed with simple sulphuret of copper, a phenomenon by no means so common in the yellow species which I have just mentioned above.

This division proposed as a standard.

This division of the sulphurets of copper, being once adopted, might be considered as a standard, to which we might refer all the numerous varieties, that exhibit no marks of crystallization; arranging them under one or other of these species, according to the manner in which their essential component parts are proportioned. We might then

from St. Bel and Baigorri, which I have noticed. In these analyses given in the *Journal des Mines*, No. 122, the author says expressly, that the iron in these yellow sulphurets was in the metallic state; while in two other analyses made of varieties of the simple sulphuret of copper from Siberia, which were probably in amorphous masses and contained iron, he says the iron was in the state of oxide.

place

place in a second division such as appear not to agree with any of those already known and classed among the species properly so called; and this division may be subdivided at pleasure, as may appear necessary for the establishment of order and perspicuity.

It is obvious, that in fact, the species existing among the sulphuretted ores of copper being perfectly known from the sum of the characters essentially necessary to ascertain them, the silver, lead, antimony, arsenic, &c., which happen to be intermingled with them, are perfectly extraneous, and do not in the least alter their essential nature. These intermingled substances once known, they may give rise to subdivisions of varieties; but these subdivisions themselves would become very numerous, if proper limits were not assigned to them. In the fahlertz, for example, from the great tendency it has to receive into its substance an intermixture of a great number of others, I am persuaded, that we should be obliged to make almost as many subdivisions as we analysed specimens.

Ext raneous mixtures do not alter their nature, but may make subdivisions of varieties.

A collection of minerals I lately received from Russia convinces me, that we are yet far from knowing all the gray sulphuretted ores, of which copper forms a component part, or in which it is simply interposed or accidental. Among the specimens in it was one bearing the name of a substance, which certainly did not belong to it; and the appearance of which, differing from that of every analogous substance that I recollected, particularly caught my attention. As this specimen affords a new and interesting variety of the simple sulphurets of copper; and affords me an opportunity of showing how we may sometimes discover, that a substance is simply intermingled with another, and not combined with it, a point frequently difficult to determine; I will enlarge upon it for a few moments.

New variety.

This substance, which is in small separate pieces interspersed in a quartz, partly compact and partly lamellar, is of a fine, close, compact grain, and of a hardness nearly equal to that of fahlertz, or gray sulphuret of copper and iron. Its colour is a duller gray, and its fracture is more smooth. Its specific gravity is 4.554. Well assured that this substance was not nickel, under the name of which it

Described.

had

Analysed.

had been sent me, I requested Dr. Wollaston, to have the goodness to ascertain its nature. His examination informed him, that it contained nothing but sulphur, copper, and antimony. Desirous of ascertaining if possible, whether the antimony were combined with the copper in it, or simply intermingled with the sulphuret of this metal; and this ore being soluble, though very slowly, in cold nitric acid; I first of all dissolved it in this acid. A part only of the sulphur rose to the surface of the solution; and it is probable, that the rest was converted into sulphuric acid. The copper dissolved entirely, and the antimony was precipitated in the state of oxide. The latter, to judge from the size of the specimen I had set to dissolve, was evidently in smaller proportion than the copper. As this same substance is extremely fusible, I brought a thin piece, about four lines long, to the state of fusion, and kept it so for a short time. Great part of the antimony sublimed, covering the surface of the body on which it rested with a white powder. The fragment when cooled retained its form, and even its bulk. On breaking it afterward, its fracture exhibited an aspect exactly resembling that of the sulphuretted copper which is produced by the last fusion, it could be cut with the same facility, and the cut had a metallic lustre. Having afterward placed this fragment, which had been fused, in cold nitric acid, and a fragment of simple sulphuret of copper along with it by way of comparison, they both dissolved slowly, comporting themselves exactly in the same manner, and the solution contained nothing but copper. The solution of each of these fragments produced the same black, flocculent, and very light precipitate, which was nothing but sulphur still united with a small portion of copper, which, no doubt, was the cause of its black colour. From these details it appears to me there can be no doubt, that the ore was a simple sulphuret of copper, with which antimony, probably in the state of a sulphuret likewise, was intermingled. This substance came from Bojojawlensk, near Catharinenbourg, in Siberia.

H. On

II.

On the Effects produced by the grafting and budding of Trees. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

WHEN I first began the study of grafting and budding by dissection, in order to judge of the effect produced in trees by such an operation, it was my design to collect all the knowledge disseminated in every author on the subject, and by joining it with what I should attain by dissection myself, offer to the public a treatise, that might at least serve as a sort of standard of our knowledge in this art. After dissecting therefore an innumerable number of grafts and buds of every different tree, commonly grafted and budded, and at different distances of time, making drawings as exact as possible, and committing my own observations to paper; I was anxious to see what other botanists had said on the subject. But great was my astonishment to find, that scarce any author had really investigated the matter. Even Miller gives only a few common rules for practice, without observations. The scientific Mirbel (in whose work I hoped to find important information) gives only a short note, and a reference to Duhamel; Malpighi and Grew are totally silent; and Dr. Smith and Willdenouw are equally neglectful of the subject: yet it appeared to me to be in every respect that which promised the most important instruction with regard to the manner in which trees are formed, to show the process of each different part, and produce evidence which no other situation of the vegetable world is capable of giving. Nor was I deceived, I think; for by this sort of dissection I have learnt more of the real nature of the different parts, than any other investigation of plants ever taught me; since they are brought forward in a state, that obliges them to exert their powers, and much may be drawn from the curious struggle for life, which points out to notice every important part.

Finding that from Duhamel alone I was to expect any information, that was not merely practical: I with great

Design of the author.

The subject not investigated by writers.

Yet highly important.

Duhamel's book.

trouble at last procured his work. It is indeed a book full of most admirable instruction, and though it has but little beyond practical observations on this subject, yet, as far as he proceeds in investigating causes, it is excellent: but here again the want of the opaque solar microscope has prevented the possibility of his proceeding farther, and knowing whether the parts did, or did not unite. This being the case, I must trust wholly to myself for the anatomical part, and the consequences arising from them; but as I shall give a sketch of the exact change made by the uniting of the two branches. I hope my figures will prove the truth of my assertions.

Object of grafting and budding.

Why seedling trees are commonly varieties.

The use of grafting and budding is to propagate any particular tree: the wood or fruit of which pleases the eye or palate; as the only means we possess of procuring a perfect imitation. It is a well known fact, that seeds seldom produce the exact prototype of the plant from which they spring; and the reason is plain; the tree is only the mother of the blossom, but to complete the resemblance, it must be impregnated by the stamen of the same plant. Now if the wind blows the ripened dust of the stamens from a neighbouring tree of the same order, when the blossoms of the first are covered with the juice of the pistil, the blossoms thus prepared will receive the powder, and the consequence will probably be a new variety of fruit, if the seed is planted; and not the same fruit which the original tree gave. Thus are produced more than half our sorts of apples, peaches, and innumerable flowers; for we neglect to examine the origin of the varieties that take place in our gardens, or we should continually be able to trace them to this source.

Budding and grafting merely continues the same tree.

But in grafting and budding, this cannot be the case, no resemblance can be so exact; it is indeed merely an increase of the tree, from which the scion is taken. Every one knows, that grafting is taking a shoot from one tree and inserting it into another, in such a manner, that both may unite closely, and become one tree. Mr. Bradley (from some observations of Agricola) suggests what anatomising the parts first proves, that the stock serves merely as pipes to convey the medicated nourishment to the scion; that the

scion

scion preserves its natural purity unmixed or uncontaminated with any other juice; and, as we have since discovered, that all sap is nearly the same, being merely the juices of the earth; and that it is the blood which runs in the bark alone which gives taste and variety to the tree; so the scion can in no way be altered by the stock, without the juice of the bark runs into it. Now it is most certain, that this is not the case; for the barks never join so as to communicate juice, as I shall show when I describe the alteration made in the parts in contact. The scion or bud (for it is the same in both) is placed on the graft or stock as in the earth; but, instead of having to prepare its own sap (which the infant plant is obliged to do before its root grows) it finds this ready medicated for it, and fitted for its more advanced state, to push it forth with a vigour truly wonderful. To graft is merely therefore to form a tree in a far quicker manner than a seed could; and in very delicate plants to take away from them all the dangers attending their infant state; and at once place them in maturity. This is the real object of grafting and budding.

No union of the vessels of the bark in graftings.

I cannot agree with Mr. Foresyth, that the juices, when they arrive at the scion, must have a more easy, plentiful, and perfect assimilation, than if they were its own; for the scion can only do well, when the vessels of the stock agree perfectly with its vessels. Its own cylinders must therefore more perfectly assimilate than any stranger tree can. Still, if the wood vessels suit each other, we may be satisfied that the plant will do well. This is indeed of such extreme consequence, that of the number I have examined which I had budded and grafted for trials of various trees, more than thirty have died from the difference of the size of the two woods; for does it not stand to reason, that, if you try to force a quantity of water from a large vessel into a small one, it will burst the small one: or, if it is the small one, that pours its contents into the large one, it will not half fill it, and the vessel will be pressed with too much air, that will pour in to supply the place of water; and the vessel will equally burst. This is literally the case in the two woods that are to be joined together, whether in plethory or emptiness of sap, they equally burst, but in the

Juices of the stock not more agreeable to the scion.

first, the whole is wet and full of juice; and in the latter, the parts are shrunk and dried up.

Grafting and budding similar in effect,

i but budding preferable.

I must now mention, that, though grafting and budding are so very differently performed, yet in their effect they are perfectly the same; the first being the planting several buds, the latter only one: but there are some reasons, that make the last infinitely to be preferred, where it can be done: and I doubt not it succeeds much oftener, as I shall show at the conclusion of this letter. I shall now proceed to the alteration effected in the bud and graft by the operation. The first observation on cutting a graft, after it has been done about two or three months, is, that all the part between the two plants is filled with a moist substance; which upon magnifying you perceive to be the same wood as the scion, only loose, and incomplete. Next a white line is seen struggling through this loose wood, and soon reaching in a very undulating manner from the stock to the scion. It always begins at the stock. To perfect this line, which is the circle of life, six weeks are required. I never saw it perfected in less.

Effects of grafting.

Junction of the backs.

The next is the formation of what Duhamel calls the bourrelet, that is a species of bolster of new bark, formed from the old juice of the stock, which, being prevented from continuing its course to form new bark, runs down the division of the two branches, and joins them with a new piece: for let the barks be laid ever so close, or even wrapped one on the other, the old barks will never join; and it is necessary, that a piece of new bark should cement the two edges. But the juice stops at the end of the join, and it is perceptible by the extreme dryness of the two edges, (so different from the rest,) that no juice passes from one to the other.

Grafting cannot improve fruit.

There is no communication therefore between the barks of the two branches, of course the bark of the scion is pure and unalloyed. How then is grafting or budding to meliorate a fruit? I believe, that there is not any thing more certain, than that it makes no change whatever; and that there is no practise that repays so ill as that of repeatedly grafting, and cutting down plants. It must exhaust, and I have heard an excellent old gardener say, who has practised

Repeated grafting is a bad practice.

tised this art for thirty years; that he, after years of repeated trial, was perfectly convinced of this truth.

The next thing to be observed in a graft or bud is the **row New wood** of new wood round the division between the bark and wood. In the common beech for stock, the scion being the copper beech, the new wood is always of a pink colour, by which means it displays the mixture formed with the wood of the stock, which is perfectly white. See (Pl. IX, Fig 1,) a graft of one year, the scion not only increased at, p. p, the usual way, but continued rounds are added till they meet; and the whole of the stock is eradicated. There is a very peculiar appearance in the wood of the scion and graft, which well proves that they can in some measure alter the direction of their vessels, even after the regular formation of the wood; it is the undulating form, sometimes absolute twisting and turning of the vessels, which Duhamel notices, and which, he adds, strongly resembles that in glands in animal bodies after a great incision. And thence he infers, that a new sort of viscus takes place, where the two branches join; which most probably must greatly meliorate the fruit of the scion. Unless Duhamel had tasted human flesh before and after amputation; I know not how he could draw such an inference on the melioration of flavour. If he found it corrected the taste of the former, he might indeed draw the same inference in favour of the fruit. Or I should suppose it was much more natural to infer, that this undulation was caused by nature being disturbed in her office, and by the struggle the circle of life makes, to pass to the new branch, which soon however subsides, a few inches higher: and as to the new viscus, when placed in the solar microscope, it proved exactly the same wood as the scion.

There has long existed a dispute with respect to the **Formation of** manner in which the bark and wood are formed, which, it **the bark and** appears to me, dissecting grafts is the true way of elucidat- **wood.** ing and deciding. It is most plain, that the bark and wood have not the smallest connection, but that which the attaching of the flower bud to the wood occasions. I have been long of this opinion, and my present occupation has confirmed the idea. I think indeed, I have a specimen, **that**

that would convince the most unbelieving, and prove, that the process of the formation of the wood proceeds in this manner. When the sap begins to rise, it detaches the rind, the bark, and inner bark, in one close layer, from the wood; and they, being disposed to grow faster than the inner part of the stem, increase as much as the fastening of the flower buds to the wood will permit. The sap then forms the new wood in the intervening space, and a band is completed each year.

Lusus naturæ.

I found some time ago a *lusus naturæ*, which teaches more than all I can say on the subject; and I have given as exact a drawing of it as I could make. See *pl. x. fig. 1.* On looking at some plants, I observed a Portugal laurel appeared in a very strange state; and on examining it, I perceived some accident had separated the rind, bark, and inner bark, in two regular bands, from the stem of the tree. Still, however, the ends were attached, but the loose part, from being at liberty, had so wonderfully increased in length, that it was more than double the measure of the piece of stem it originally covered. It had broken the trifling hold of the nourishing vessels, and that of the flower buds, which, when I found it, were perfectly dead, but still it had thrown out its leaves, and was forming fresh ones. On dissecting the leaves, there appeared no nourishing vessels, or their emptiness prevented their being distinguished; and the spiral wire was only to be found now and then, and in a broken and dilapidated state. I deeply regretted the having separated the branch from the stem, before I knew what it was. But it is still a very great curiosity, and explains well the powers of the different parts. It plainly marks, that the rind, bark, and inner bark, are the formers of the leaves; and that though they receive most part of their nourishment from the nourishing vessels which spring from the wood, yet they can expand and form without them. They did not indeed appear in perfect health, nor could it be expected, as their whole nourishment came from the dew they received, and the carbonic acid gas they inhaled. The inner bark vessels were full of the blood of the plant, and did not appear to evaporate in any manner, though one side was exposed; which shows how very complete must be the separation between the blood of the plant and its sap.

It

It strongly indicates too, how impossible it is to gain a thorough idea of the juices of trees, since to procure them they must be mixed; and no person can, I think, dissect trees without perceiving the astonishing pains nature takes to prevent this mixture, which would probably render futile all the intentions of nature. How then are we to judge of them, when we get them only by wounding the tree? in barking we get two juices of a very different nature, for that in the rind is purer than the sap in general, and often very bitter; then there is the juice of the circle of life, which is clammy, and approaching to syrup; and an almost plain water, that is often to be found concealed between the folds of the pith. All these should be procured singly, to be able to understand them.

I shall now return to the grafts: having described as minutely as possible the manner in which the two branches of the grafts join, I shall mention also, that the woods as well as the bark must have a new piece to join the wood of the stock and scion together; as will be seen in the plate. It often happens that the white undulating line before mentioned, which is the line of life, a little intercepts their meeting, but this is soon conquered; the line of life is always to be traced from the pith of the stock to the pith of the scion, as if to establish the communication of life, which adds another proof to those before adduced in my 4th letter, (*See vol. xxiii, p. 334,*) that it is the most important part of the plant, and truly what I have named it, the circle of life, or propagation. In looking over Duhamel, I was not a little pleased to see he had marked its consequence, but was uncertain what to call it.

I shall now mention the folly of expecting heterogeneous mixtures in grafting or budding to succeed, black noses, &c. That a plant should be capable of receiving its nourishment through the cylinders of another plant, is astonishing; but it must at once appear how much this miracle must be increased, if two plants are taken, which in their nature are wholly different. That such a mixture may be made by applying the powder of the stamen of one plant to the pistil of another, I know, but not in the way of graft:

Difficult to attain a knowledge of the different juices of trees.

Wood of grafts requires a new piece to join them.

Heterogeneous mixtures in grafting or budding cannot succeed.

that new grafting an old tree, and cutting it down, may make it bear fruit, when it would not before, I believe; because to cut and pare a tree always infuses fresh vigour into it; for the momentary flow is more hasty, it has the power therefore of clearing away all intervening objects, that might prevent the more perfect flow of the sap; but all this is very different from allying two objects of a distinct nature. Resolved, however, not to trust any thing to reasoning alone, I grafted many of these trees myself, and got an excellent gardener to graft and bud a number. Most of them died before the year came round to cut them; but a chesnut on a palm lived much longer, though it was decaying. Some died because the juices of the circle of life coagulated with the juices of the pith on meeting; at least there was a strange substance, that had much this appearance: others died from the irregular size of the wood. But I have a number now, that I mean to give a more exact account of than I am at present prepared to do; which will illustrate this subject in a manner to leave, I hope, no doubts concerning the impossibility of joining these heterogeneous mixtures, which nature never intended should meet.

Wood.

I shall now close my letter with some observations concerning the wood. The newly inflating the dead wood is not peculiar to grafts; for it is seen in the spring in many plants, as was exemplified in one of my letters, in which I showed how the graft was revived. The dissecting of grafts has at least this advantage, it adduces fresh proof of the simplicity of the formation of the wood, and showing it to consist entirely of cylindrical passages for the current of sap. These can not only be inflated with other juices as well as their own, but the wood will remain for many months in perfect form, without decay, though they are empty.

Hydrangia.

The hydrangia exemplifies this each year in a way so curious, that it is worth laying before the public. The whole of the stalk dies away, except the cylinders of wood, the rows of pith, and the rind, so that there remains a total vacancy between the rind and wood. The bark and inner bark decay, and fall away to powder. The life dies down to the earth, and there remains in a torpid state, till a few fine days
in

in the spring revive it, and give it strength to shoot forth, and run in its few simple vessels up to the top of the dead wood. Warmer weather throws up the sap to the top of the old wood vessels, by the pressure of air below; but the vacuum existing between the rind and bark prevents the juice filling the last row or two of the wood, and gives it the appearance of a fresh rind, which I at first took it for; but on placing it in the solar microscope, I found it was the last row of the wood. If you take off the rind at this time, it will be found standing hollow, with an apparent rind skin on the wood. After a time the blood of the plant begins to form; though in what manner I have not the most distant idea, for it does not appear to me to have any connection with the root. I do not however say it has not: but I am now deeply studying this part, which is that I know the least of. I hope however to make it my winter study, as I have been collecting specimens for more than two years for the purpose, and have now a large collection of drawings from which I have not yet been able to take the results, for want of time, and the interference of grafting and budding. As soon as the blood is formed, the bark and inner bark begin to grow, and I was surprised to see, that they grew exactly in the same manner as the bourrelets in the graft (see *Pl. IX*, fig. 6 and 10.) At first I could not reconcile this form to the usual form of the growth of the bark, which is much the same in most plants; but it is its inflated appearance, that disguises its natural shape, and hides part of the lines as is seen at *dd*. It appears very evidently to prove, that there is no return of the blood of the plant: it will not then be called a circulation. I cannot therefore agree with the gentlemen, who did me the honour to notice my letters in the Panorama I think, but I have not seen it, a friend having copied this remark to send me.) "That the holes in the cylinders of the blood vessels were intended to prevent the return of the blood, or vegetable juice." Now there being so little motion in a plant is the very reason, I should suppose, why it will be the less likely to run contrary to the manner in which it was intended; and I do not believe, that there is any circulation in this part.

But

Plants and animals should not be compared.

But ingenious as the idea is, there is not any thing, I confess, that appears to me more fallacious, than the comparison between the animal and plant; or that causes more mistakes. The one has action, the other only motion; the one has volition, the other is a mere machine. It would at first sight be more to the purpose therefore, to compare it to any of our works of this kind, if the plant had not life, which must render every comparison futile. In the infant plant there is some resemblance to an animal; but it has caused more errors, and more delayed the progress of phytology, than any fashionable idea I am acquainted with. Where find a comparison for that, which is evidently a machine, but one that has life without volition; that is formed with such ingenuity as to combine its powers, extract its juices, decompose, and recombine its gasses, and deduce from a combination of the whole new life, and new beings? Mirbel alone has classed it in a manner worthy of itself. "The vegetable world," (says he,) "is the division between the organised and unorganised parts of creation: it gives life to the unorganised substances of the Earth, changing them into living vegetables, for the support of animal life."

I am, Sir,

Your obliged Servant,

AGNES IBBETSON.

Cowley Cot, 22d Oct.

III.

On the Defects of grafting and budding.

By MRS. AGNES IBBETSON.

To Mr. NICHOLSON,

SIR,

Important in grafting, that the wood of the stock and graft should be similar.

I SHALL now continue my subject. The most important part toward making grafting and budding perfectly succeed is, that the woods of the stock and scion should exactly assimilate, and be enabled intimately to connect with each other. To prove how very necessary this is to the joining
of

of the two parts, I have given a sketch of woods of different patterns, to show the impossibility of their meeting and carrying on the circulation, unless they agree in every respect; and that each contains a pretty equal quantity of sap. (See *Pl. X, Fig. 2, 3, 4, 5.*) Conceive each of these circular apertures to be cylinders, and supposing a quantity of similar vessels laid close to each other in rows, and that for the use of some machine a double quantity of vessels was wanted, to make it double the height, to convey the liquid on through this whole series; would it not be most apparent, that the vessels so applied for the continuation of the pipes should be formed of the same size, and of the same shape, as the first? and that, if they were either larger, or smaller, the liquid would escape in pouring from one set to the other? Just so it is with the cylinders in the wood vessels of the stock and scion: except that, having no apertures from which the sap can escape, it bursts the under vessels, that should contain it.

The next thing necessary is, that the time for the flowing of the sap should be nearly the same both in the stock and scion; for every tree has its own peculiar period for this operation, and the energy with which it acts at that time is to be traced to the most distant leaf. The current is then very great, and the effort it makes is infinitely superior to its motion on any other occasion. If any obstruction has taken place before in the inner part of the tree, either by the introduction of a worm, or the piercing through of any cryptogamia (no uncommon accident) this is the clearing time; as the tree has then strength enough to force away all heterogeneous matter, that hurts or impedes its growth. If therefore the scion should advance to this period before the stock, it will want the sap to act with, and be debilitated and starved, till the time for the stock arrives, when it will have lost its energy: and if the flow of the sap takes place in the stock first, the vessels of the scion will not be ready for its reception, and it will probably burst them. I have often found it in this state. In the one case the scion appears shrivelled in its upper leaves, in the other it leaks at the graft, and decays often by slow degrees.

The

Rain and strong
sunshine to be
avoided.

The third point is, that there should be no grafting or budding in rainy weather, or in very strong sunshine. The first is apt to swell the graft and buds, and overload them with moisture, so that they are killed with plethora; and too much sun dries up the bark and rind, and draws off the moisture, so that the bark draws itself up from the bark of the stock, and they are forced asunder. The gardener imagines it is owing to the thickness of the rind, whereas it is generally his own fault: many disappoint themselves, because they cannot bear to leave off, when they have once begun.

Nurseries of
stocks should
not be over
crowded.

Great care should also be taken, that the nursery, from which the stocks are brought, is not over crowded, and yet this appears to me to be little thought of. No one, who does not dissect vegetables, or trees in particular, can have an idea how much they are the children of habit. One vessel by the accidental pressure of another, or some trifle, gets a twist, which is followed by the next, and so on by a third; the daily impression continues; the plant grows stiffer, and therefore more incapable of being righted; till the whole tree takes a spurious shape, from so trifling and minute a cause, that no one would credit it, if remembered. There is not any thing more beautiful than a well grown tree, if proper pains were taken to make it straight, and well shaped. To graft it, is certainly to give it a defect; but if well grafted, the injury is small, as it should be little increased in size at the grafting place. A grafting or budding, that can be easily pointed out six or eight years after, is badly performed; and one that enlarges the tree where it is budded is badly done. If gardeners would encourage their grafters to be double the time about it, they would find their account in it. The smallest quantity of air introduced increases instead of banishing the rot already there. In cutting 120 grafts, and 90 buds, more than 70 of the grafts had so much rot in them as could not be banished without great care; for when there is rot, if a severe frost comes, and this part is not guarded, the frost attacks it, and the decay increases. The first proof of this is the shrinking of the most distant branches.

There

There are very few grafts entirely without rot. The Few grafts with-
 example I have given at *Fig. 1, Pl. IX*, has less defect ^{out more or less}
 than is usually found, the parts *ff* are trifling degrees of rot.
 decay, and would soon have been banished, if kept from
 the air. I know but one way of doing this, which is *Remedy*.
 by placing a composition on the place, when the clay or
 bass mat is taken off. I know none better than *Foresyth's*.
 Any composition that will keep out the air, and not crack,
 will do: but his having been tried, and warranted by such
 competent judges, must have the necessary qualities one
 would suppose. I knew a gentleman, who always covered
 his trees, whether grafted or wounded, with a plaster of
 this kind; and his grafts and buds were truly a picture.
 He did it for two or three years, according to the appear-
 ance: a bud but half the time. Within this space all
 danger is over. It is a method that would save thousands of
 trees, if adopted. If the number of grafts, that die between
 the 3rd and 6th year, were counted, they would perhaps be
 found nearly one eighth of those that survive the first
 operation. I have a collection of grafts between those ages,
 that well show the danger arising from this constant in-
 crease of rot: which will almost inevitably take place in
 every graft, when first performed, if not well guarded from
 the air: but if, when the clay is taken off, the plaster is
 put on, and renewed every six months for two years; or,
 if delicate, three; all danger would be at an end. The
 joining of the bark would have been renewed by that time,
 and taking off the plaster by degrees, the rind would be
 fresh and hardened.

On the contrary the method of performing the operation *Common mode*
 is this. A certain time is marked out for it at most *of grafting*.
 nurseries. The weather is little attended to, either in grafting or bud-
 ding; because the hurry of business will not admit of such
 nicety. It is supposed, that, if the shoot takes, all is
 well: but not one in a thousand is really joined when the
 clay is taken off. The operator should have a common
 little microscope, which would show him, that they are so
 open as to admit air enough to destroy half, though to
 common observation they appear closed. In this state they
 are prepared for selling. Some will live two, three, or four
 years;

years; but if cut they would show, that they carry their certain death with them. The gardeners indeed may say, to manage them in this careful manner would take so much time, as would ruin us; for the price is not adequate. Certainly not; but it would better answer to a gentleman, to give half a guinea for a good tree, that would live; than buy ten for a shilling a piece, that will die just when they should bear well; and many, I dare say, would prefer it. I have this year cut two to endeavour to discover the cause of the decay of the apricot, particularly the Anson, which loses a limb each year. In vain I searched every part for the defect, till I came to the graft, and then it was visible enough: for the separate parts of the wood had decayed just as it led to each of the large branches, till there were but two left. The canker very usually begins there.

Decay of the
apricot.

Canker.

Grafted beech.

I was examining a small copper beech tree, grafted on the common, six years old. I thought it appeared sickly from the uncleanly appearance of its leaves; for it is certain, that when a tree grows unhealthy its juices grow sweeter; and the insects therefore seize it with double avidity. This had its beautiful leaves much disguised with the filth of the vermin that swarmed on it. I examined every part, till I observed a great enlargement about the graft. The rind was loose, and on making an incision, I found the bark all decayed to powder, half way up the tree; and I took from it above a pint of woodlice. On examining it had certainly arisen from the rot in the graft: which had never been well joined; and which had soon allowed these creatures to form themselves a habitation between the two barks. This spread by degrees; and had I not discovered it, the tree would soon have died; but by a proper application of the composition, and cutting away all the decayed parts, I doubt not it may do well. I was not a little surprised to see Mr. Foresyth advise the cutting three inches above the bud, which is certainly leaving great room for rot to accumulate.

Budding.

I shall now mention the difference between budding and grafting, and the reason for giving a preference to the former. There is in my opinion no comparison between them, so infinitely superior is the practice of budding.

The

The chance of escaping the rot is much greater. As I prefer experience to reasoning, of 90 buds that I cut, only 30 had any rot within them; but in 120 grafts near 60 were so bad, as to require care to banish the evil from them. Then the bud stands a far greater chance of taking, for it lies much closer; and there is almost always (especially in peaches, nectarines, apricots, &c.) a couple of concealed buds, which are sure to succeed, if the middle one fails. Beside this, the line of life, which ensures their joining, is more quickly completed in the bud than in the grafts. In the former three weeks or a month will perfect it; but in the latter six weeks are the earliest time, and I have known it three months. A bad bud can hardly be so ill done as a graft.

It is a great pity the country practitioners will not bud their apple trees, for it is really mortifying to see what mischief they do their trees in grafting them. I have many examples by different bunglers around me, that make me deeply regret this, on account of the cider: and much of the yearly ill bearing may come from this cause: for it is making the tree so delicate, as to have its sap checked by every change of weather. People who have not studied this subject may think I exaggerate the evils, that may arise from it; but the custom that I have followed for a few years of dissecting every old fruit tree I could get, and of seeking in every dead bough for the cause of its death, has laid open to me many evils of this kind little thought of; and I hope shortly to give a letter on the decay of trees in general, that will prove I have not advanced more than experience warrants. What I have now written is not as advice to well established nurserymen; they, I doubt not, manage in a far better manner; I have not the vanity to suppose I can give counsel, where experience must be so good a master. But in this remote country with common practitioners we are certainly not so clever; and in many of the smaller nurseries about London I saw miserable examples of ill management in this art: and in orchards in general, if people would adopt budding, or, if they must graft, make use of the plaster for a year or two to their grafts; and throw the soap and water, with which they wash

Trees should be washed in their families, on their apple trees to cleanse them from vermin.

If they would get a man once a year to scrub them with a hard brush and urine a little diluted, as one man could do a middle sized orchard in a day, the expense could not amount to 6d a pipe, and the quantity of cider would infinitely repay the trouble. I am acquainted with a gentleman, who tried it with one tree; and it bore every year in a surprising manner. But the quantity of cryptogamian plants, that is allowed to draw off the nourishment from our apple trees (if calculated) would scarcely be believed. All these parasite plants throw their roots into the trees, as the trees do theirs into the earth; and equally draw forth their support from them. There are many thousands on each tree; they flower and fruit in a short time; and all the juice for these purposes must be drawn forth from the tree. Does it not therefore stand to reason, that the tree will be weakened, and the fruit lessened by this draining? Proceed in your examination of the orchard: and view the lumps and excrescences to be found around the grafts. If cut, they will prove full of vermin. They also assist in draining the tree of its nourishment. We constantly manure, when we place in the ground seed, from which we expect some return. This is done in every case; except in orchards. Why should they alone give all, and receive nothing? The consequence is plain. They have what they call a good year but once in three. I once knew a butcher, who had an orchard that yielded many pipes every year. He was said by his neighbours to be a lucky man, and envied accordingly. I inquired into his manner of managing. A few times in the year he diluted some fresh blood (bullock's I believe) and poured it on the roots of his trees; covering them with a fresh layer of earth. This might take him probably four or five days in the year to accomplish; and his orchard yielded him double the quantity it was in the custom of doing before this practice. There is perhaps no gain so sure as that we give to the earth: it always returns fourfold.

Juncture of the graft and stock. I shall now endeavour to explain the sort of bolster, or bourrelet, which joins the stock and scion. It is of the greatest consequence to the tree, and influences, I doubt not,

not, the sort of tree produced; as I have before endeavoured to prove. For if the resins of the two trees mix, I have, I think, given the most convincing proof, we should experience this in the taste and appearance of the fruit. I know no two barks, that differ more than those of the plum and peach in taste and appearance. I took a fresh graft before the rind had covered it, and cutting off the bolster, I formed a very thin slice, and placed it in my microscope. The whole part represented at *nn Pl. IX, Fig. 7* and *8*, proved plum bark: but the under piece (*mm*) was certainly peach bark; and though I have examined it completely, as long as the rind is covering the whole, they never join. One edge lies over the other; but no vessels pass between the two, and there is no communication whatever. The fruit of the scion therefore must be pure, and wholly the produce of the peach tree, and the fruit can in no way acquire melioration from the mixture with the stock. In budding the juncture is made exactly in the same manner as in grafts. But as, in spite of appearance, the bark really never joins, it may easily be conceived how very necessary a plaster must be, till the new rind has grown over the whole; and this depends upon the thickness and height of the bolster, and therefore of course on the grafting or budding being well or ill done. If the air is allowed to enter the smallest pin hole, and the budding or grafting has left the smallest rot, the aperture will make it a serious evil.

I have observed a curious circumstance which frequently occurs respecting the side buds in budding, though never in grafting. Growing fast, they will often take refuge with their new made wood under the cover of the bark of the stock, and make the bolster between the buds: still they never join.

Curious circumstance in budding.

There is a defect in grafting, which is sure to cause death. This is when the stock and graft differ in size; and the graft is not placed even, so that the ends of the bark vessels of one come into contact with the wood vessels of the other, permitting the juice of the bark to run into the sap. Here the sap retires, and a black rottenness takes place; which proves how necessary it is, that the juices should be divided; and shows why nature has taken such

Fatal defect in grafting.

pains, to carry up all the juices separate to the flower and fruit. (*See my former letters.*)

Juncture of the wood.

As to the grain of the wood, from all the specimens I have cut it appears, that the bastard grain must be joined; and that the twisting and turning of this, when it could not join, (see *Fig. 4, G n*) was the cause or rather perhaps the effect, of the death of the graft. This has always been the appearance when the graft and stock did not agree. But as to the silver grain, it appears of little importance whether it joins or not: for I have seen it not meet in very perfect buds, that completely succeeded.

Shoulder grafting.

I shall now mention the different sorts of grafting. When old trees are grafted we must do the best we can: I should think shoulder grafting preferable. It would be in vain to notice the defects of that which is to make a thing worth something, that was worth nothing; though it can never make perfect trees of them. If however the plaster is put on, when the clay is taken off, and the graft is well staked; it will succeed, and make tolerably good trees.

Grafting by approach.

As to the grafting by approach, it would certainly be the best kind of grafting, but for our climate. I know a gentleman, who had practised this in India with the greatest success, and he had so completely the perfection that constant practice gives, that I was very anxious to watch the effect of his trial in this country, as he did not seem to imagine it would not equally succeed. He began; but soon found his success very different. The sap, that should have formed his grafts by making new wood for the purpose, was stopped by a few cold days. This introduced the rot into his scion. To prevent this, he next began his operation in very warm weather; but this had a bad effect on the barks, and made them recede from one another, which was sure to destroy them. In short after two years trial he was convinced, that, except for a very few plants, the sap of which was very difficult to be backened, this was not a climate for the purpose. I am not quite satisfied however to leave the result in this state. Resolved to be assured of the reason of its frequent failure, and, if it does succeed, of the poorness of the tree that proceeds from it, I have got several done by a person who in general succeeds un-
usually

usually well. It may be, that both bringing sap will not do so well, as it does not so completely make the stock a passage for the sap. Nothing but experiment however can prove the truth of these conjectures, which I hope to verify.

I fear the Chinese method would not do better though something different from this. (*See Trans. Soc. of Arts*, vol. xxv, p. 14, or *Journal*, vol. xxii, p. 321.) It would I think want more than two months, or even four, to strike a root fit to nourish such a branch. We know how large a root is necessary to support an infant shoot: and I much doubt whether such a branch would not bleed to death, especially in the spring, and unprepared. This time of the year, October, seems much more fitted for the trial. But I must think in one respect the gentleman must have been mistaken: that it fruited the better for the cutting. In our climate the flower bud is formed the autumn preceding its fruiting. I doubt not, that vegetation is much quicker in China; but not sufficiently hasty to produce bud, flower, and fruit, in two months; which must be the case, if the operation of abscission causes more fruit to grow on the branch, than was there when chosen. As to the fruit being larger and better for taking off the leaves of a plant, it has been too often tried, to allow of our being again deceived. We know, that the fruit decays on the tree being wholly robbed of the leaves; and how should it be otherwise? it must at once lose all the carbonic acid gas, that passes into its circulation; and all the dew, that is taken in by its leaves, is decomposed and produces the oxygen for us, and the hydrogen for the seeds. Is it therefore to be credited, that in any climate plants would be the better for such a change in their formation? I beg Marsden's pardon for believing he has made some mistake.

Chinese method.

Fruit trees not to be robbed of their leaves.

The most common way of grafting is whip grafting, or tongue grafting, which is certainly the best that can be followed in this climate. But there is in the practice of this a custom, which would be better exploded; the giving the notch. It is said to hook them to each other; but this is a great mistake; for that part is soon in a manner reduced, and I have known it frequently introduce the rot into the graft.

graft. Indeed as to fastening it, it cannot do so; for if it does not decay, it is so steeped in sap, and so watery in the new made wood, that it is impossible it can have any strength. It is very certain, that, where the knife is not absolutely necessary, it does the greatest mischief. I have seen gardeners also go on with the same knife from graft to graft, and do great damage in this way, by the decomposition of the iron, and the dirt the inside of the graft contracts. It is impossible to conceive how many of these trifles act to the serious detriment of the plant. All these things, the last excepted, budding escapes. I have seen awkward grafters put the scion smaller than the stock. This is a serious evil indeed; for the very way, that nature takes to counteract a plethora in the scion, introduces the rot into the stock. It is a very curious provision of nature; or rather perhaps the consequence of the smallness of the scion; but such a quantity of decay takes place, as will leave perfect the size of the scion (See *Pl. IX, Fig. 3.*) The rules of grafting may be found in any common gardening book, I mean not therefore to trouble you with them, but only with the remedies, that, if applied, might cure the defects which the cutting so many grafts has made me perceive. The stock should be cut sloping from the scion, and as close as possible, and the plaster should cover the cut as well as the graft. The buds excepted of course.

I am, Sir,

Yours &c.

AGNES IBBETSON.

Coxley Cot, 20d October, 1809.

EXPLANATION OF THE PLATES.

Plate IX, *fig. 1.* Section of a graft of the copper beech of the second year on a common beech of the third. *aaaa*, the circle of life leading from the pith of the stock to the pith of the scion. *b*, the pith of the stock. *c*, the pith of the scion. *dd*, the projecting pieces of the bark and rind. *fff*, trifling degrees of decay where the wood has not united. *ppp*, the line of new wood, which is first formed by the copper beech, colouring the sap of the stock, and making it rather pink: which proves, that the new wood gains ground

Fig. 1. 2.

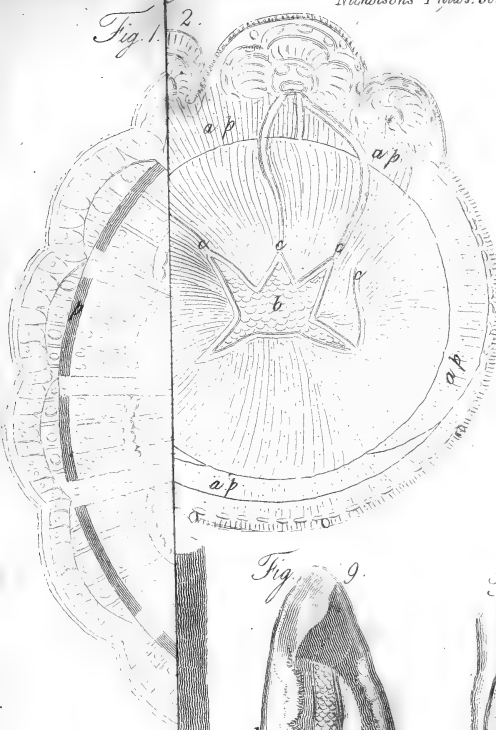


Fig. 3.

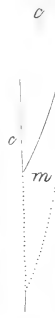


Fig. 9.

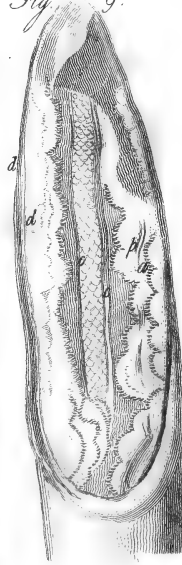
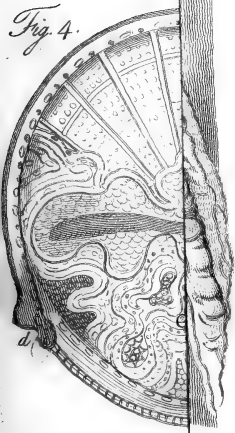


Fig. 10.



Fig. 4.



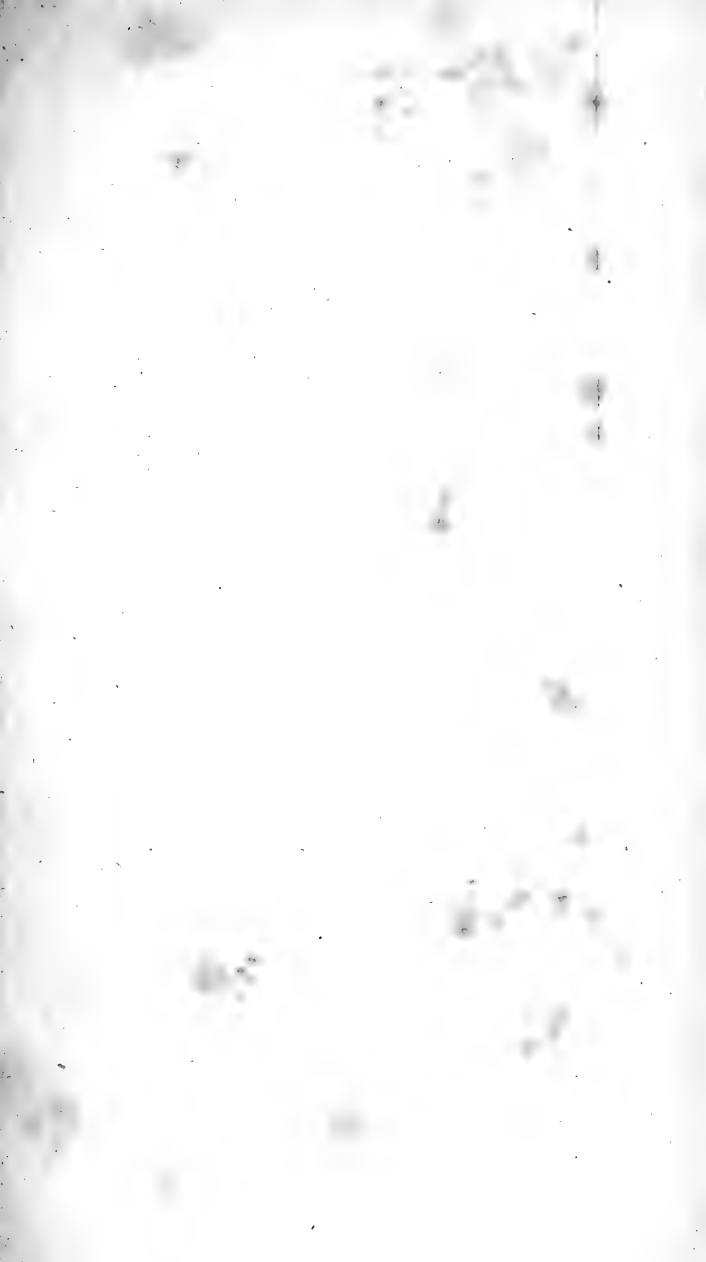


Fig. 1.

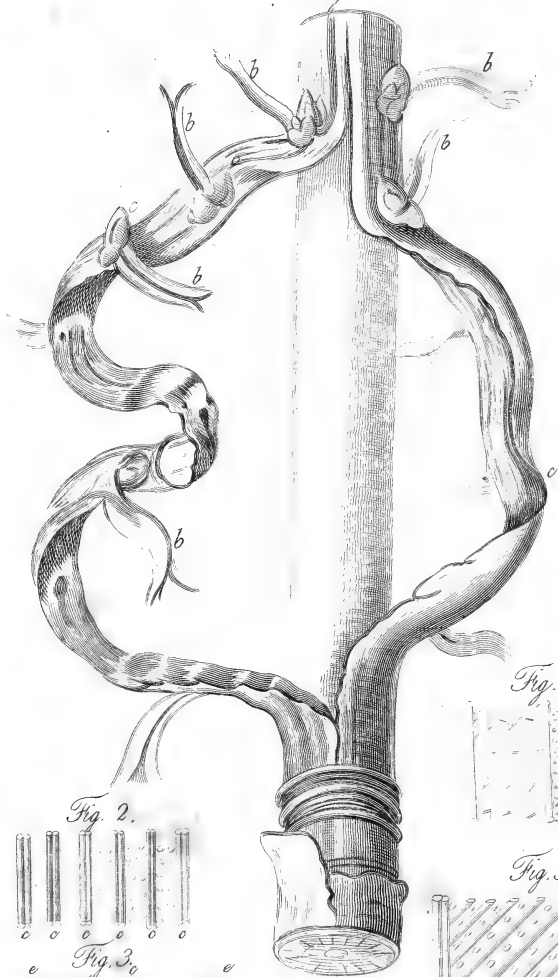


Fig. 2.

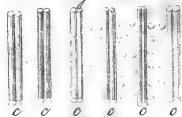


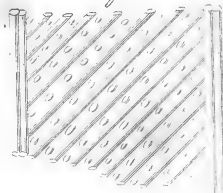
Fig. 3.



Fig. 4.



Fig. 5.



ground not only from the centre to the circumference, or from *a a* to *p p*, but that it works the contrary way, or from *p* to *a*; thus exploding the stock both ways, at least with respect to the wood.

Fig. 2. A very well joined bud. *b* the pith. *a p*, *a p*, the new made wood. *c c c c*, the line of life.

Fig. 3. The manner in which the graft is cut in this country, if the scion is larger than the stock. *c* or *c*, is the manner in which the stock is left; and *m* is generally the part, that becomes rotten, to decrease the quantity of sap by degrees.

Fig. 4. A graft that disagreed with its stock. The twisting of the bastard grain and vessels in the wood of the scion from this circumstance is shown at *Gn*. They ought to have appeared as in *fig. 5*.

Fig. 6. Appearance of the bourrelet, or bolster, that joins the stock and scion, during its formation. *d d d*, the inflated parts, that tend to conceal the fibres.

Fig. 7. The manner in which the stock and scion, whether graft or bud, are joined together, *n n*, the part which is always formed by the stock. *m*, the under part formed by the scion. The part *n n* folds over this, but never joins it.

Fig. 8. A very bad graft. The letters of reference as in *fig. 7*.

Figs. 9, and 10. Oblique section of a graft. *Fig. 9*, the stock. *Fig. 10*, the scion. *p p p*, new wood. *a a*, the new circle of life. *c c*, the old, which dies away. *d d*, the beginning of the new bark, formed in the manner shown at *fig. 6*.

Pl. X. Fig. 1. A *lusus naturæ*. *c c c*, the buds. *b b b b b b*, the leaf stalks. The leaves were on it, but I was fearful of crowding the drawing. There were many stalks also full of young leaves, growing from the buds. All the flower buds were dead; but the leaf buds were increasing, and bursting out into leaf. There was every reason to presume, on dissecting the leaves, that the bark had been torn from the stems ever since the commencement of spring.

Figs. 2, 3, 4, and 5 exhibit the difference of structure in several kinds of wood, to show the necessity of the stock being of the same nature with the graft.

IV. Experiments

IV.

*Experiments on Ammonia, and an Account of a new Method of analyzing it, by Combustion with Oxigen, and other Gasses; in a Letter to HUMPHRY DAVY, Esq. Sec. R. S. &c., from WILLIAM HENRY, M. D. F. R. S., V. P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester *.*

MY DEAR SIR,

I Should sooner have communicated the account, which you are so good as to request, of my further experiments on the decomposition of ammonia, if I had not been anxious to obtain, by frequent and careful repetition of them, results not affected by any of those numerous causes of error, which easily insinuate themselves into processes of so much delicacy. You have already been informed, that the fact, which I lately mentioned to you, (tending to prove the existence of oxygen as an element of the volatile alkali, by the discovery of oxygen gas in the products of its analysis) is not entitled to confidence, owing to the admission of a small quantity of atmospherical air, in a way which was not at all suspected. Frequent repetitions of the same process, under circumstances wholly unobjectionable, have fully satisfied me, that no portion whatsoever of oxygen gas is evolved by electricity from ammonia, even when, by means of an apparatus constructed for the purpose, the only metallic surface, exposed to the gas, consists of the sections of two platina wires, each $\frac{1}{50}$ of an inch in diameter, the wires themselves being enclosed in glass tubes which are sealed hermetically round them, and then ground away, so as to expose only the points. Nor does any difference in the nature of the products arise from electrifying the gas either under increased or diminished pressure, the latter of which, it appeared

* Philos. Trans. for 1809, p. 130. This letter, in its original form, was read to the Society, May the 18th, 1809, some new observations were added, and some corrections furnished by the author, in consequence of subsequent experiments made in June; it was transmitted to the Secretary for publication, July the 10th.

to

Supposed evolution of oxygen gas from ammonia.

to me probable, from the known influence of elasticity in impeding the combination of gaseous bases, might prevent the oxygen of the alkali from uniting with hydrogen to form water, and occasion the expansion of both into the state of gas.

Having failed, therefore, to acquire, in this way, proof of the existence of oxygen in the volatile alkali, I was next led to seek for some unequivocal mode of evincing the production of water by the same operation; a fact, which would be scarcely less satisfactory in establishing oxygen to be one of its constituents, than the actual separation of oxygen gas. The most careful observation of ammonia, during and after the agency of electricity, does not discover the smallest perceptible quantity of moisture. In order, therefore to subject the gas to a satisfactory test, I had recourse to the following contrivance. Ammoniacal gas, I had previously found, may be so far desiccated by exposure to caustic potash, as to show no traces of condensed moisture on the inner surface of a thin glass vessel containing it, when exposed to a cold of 9° Fahrenheit; though the recent gas, by the same treatment, is made to deposit water in the state of a thin film of ice. A glass globe, of the capacity of between two and three cubical inches, was filled with gaseous ammonia, which was then dried by sticks of pure potash, fastened to pieces of steel wire, so that they could be withdrawn, after having exerted their full action. This point of dryness was ascertained by applying æther, or a mixture of snow and salt, to the outside of the globe. By means of a peculiar apparatus, the gas was next strongly electrified, and the cooling power was again applied to the outer surface of the globe. Attempt to produce water from it.
Ammoniacal gas dried by caustic potash,
and then electrified.

In the first trials, that were made with this apparatus, water certainly seemed to have been formed by the electrification of the alkaline gas; for the same portion of gas, which was not affected by a freezing mixture before the process, gave evident signs of condensed moisture, when the cooling power was applied after long continued electrification. The appearance was not only quite satisfactory to myself, but to Mr. Dalton, and several other chemical friends, to whom I showed the experiment. Finding, however,

but varying in degree. The experiment repeated with great precaution.

Little or no moisture.

Avidity of ammonia for moisture.

ever, that the appearance varied as to its degree, I was induced to repeat the process with redoubled precaution; filling the globe, previously heated with hot mercury, and drying not only the quicksilver, but the iron cistern which contained it, by exposure to long continued heat. The electrified gas now betrayed no signs of moisture on the application of a temperature 20° of Fahrenheit; and gave only the smallest perceptible traces, by a cold of 0° or a few degrees below. I cannot help suspecting, therefore, that the moisture, manifested in the earlier experiments, was derived from the mercury or from some extraneous source, and was not generated by the action of electricity*.

The avidity with which ammonia retains moisture, and again absorbs it when artificially dried, is very remarkable. A confined quantity of common air may be completely desiccated, in the space of a few minutes, by pure potash, or by muriate of lime; so that no ice shall appear in the inner surface of the containing vessel, when exposed to a cold of -26° of Fahrenheit. But ammonia requires exposure during some hours to potash, to stand the test even of 0° Fahrenheit; and a single transfer of the dried gas, through the mercury of a trough in ordinary use, again communicates moisture to it. Muriatic acid gas, freed merely from visible moisture, deposits no water at the temperature of 26° Fahrenheit. This is probably owing to its strong affinity for water; for electricity, after the full action of muriate of lime, evolves, as I have lately ascertained, about $\frac{1}{35}$ its bulk of hydrogen gas, the recent muriatic acid gas giving about $\frac{1}{14}$ th after the same treatment†.

From

* It may be objected, I am aware, that as the gasses produced from ammonia are nearly double its original bulk, they may hold in combination any water that may have been generated by electricity. But though this supposition may explain the nonappearance of *visible moisture*, it does not account for the inefficiency of a powerful cooling cause to discover traces of watery vapour: for this is a test which renders apparent very minute quantities of water in gasses.

† In a course of experiments, which I have described in the Philosophical Transactions for 1800, it appeared that muriatic acid gas, after being dried by muriate of lime, gave nearly as much hydrogen

From the average of a great number of experiments on the decomposition of ammonia by electricity, I was for some time led to believe, that you had rather understated the proportion of permanent gasses obtainable from it by this process, (viz. 108 measures of permanent gas, from 60 of ammonia, or 180 from 100). For the most part, I had found the bulk of ammonia to be doubled by decomposition, even when the gas was previously dried with extreme care. In one instance, a small bit of dried potash was left in the tube, along with the ammonia, during electrization, with the view of its absorbing water, which I supposed, at that time, to be generated by the process. In this case, 59 measures, (each = 10 grains of mercury) became 115. The following table shows the expansion of various quantities of ammonia.

Exp.

1.	60	measures of ammonia, gave permanent gas	112
2.	60	- - - - -	120 nearly double
3.	59	(potash being left in the tube) - -	115
4.	55	- - - - -	115
5.	75	(under the pressure of half an atmosphere)	150
5.	65	- - - - -	130
7.	65	- - - - -	130
8.	53	(one of the conductors being of steel wire)	106
<hr/>			<hr/>
	492		978

hydrogen by electrization, as gas which had not been thus exposed. I was not however aware, at that time, of the extreme caution necessary in experiments of this kind; and was satisfied with transferring the acid gas from a large vessel, in which it had been dried, into the electrizing tube, a mode of proceeding which I now find to be quite inadmissible. The action of muriate of lime, which has undergone fusion, on muriatic acid gas, is rendered very sensible, when considerable quantities are used, by the evolution of much heat, and by a diminution of the volume of the gas. Ammonia, also, is contracted in bulk by dry caustic potash. Muriate of lime cannot be employed for its desiccation, since this substance rapidly absorbs the alkaline gas, even when the gas has been previously exposed to quick-lime. In this case, the ammonia attracts a portion of muriatic acid from the earthy salt, agreeably to the law of affinity, which has been so ably illustrated by Berthollet.

and

and 492 : 978 :: 100 : 198.78. These proportions, you will find, correspond very nearly with those long ago stated by Berthollet *, who converted 17 measures of ammonia, by electrization, into 33 measures of permanent gas, which is at the rate of 194 from 100 †. Having lately, however, carried on the process with the observance of additional precaution, (the mercury being first boiled in the tube, before admitting the ammonia, and still remaining hot when the gas was passed up), I have obtained from the alkali less than double its volume of permanent gas, viz. 280 measures from 155, or at the rate of 180.6 from 100. The variability of the first set of results arises, I believe, from the uncertainty of the quantity of ammonia decomposed. For if the smallest portion of moisture remain in the tube, a little ammoniacal gas will be absorbed, and will be slowly given out again as the electrization goes on, thus rendering the actual quantity submitted to experiment greater than appears. It is probable, also, from a fact which I shall afterward state, that mercury itself, unless when heated, may absorb a small portion of alkaline gas.

But this probably above the truth.

Proportions of hydrogen and nitrogen.

The proportion of the hydrogen and nitrogen gasses to each other in the products of ammonia decomposed by electricity, I am satisfied, by recent experiments (June, 1809) is as nearly as possible what you have determined, viz. 74 measures of hydrogen gas to 26 of nitrogen. The nearest approximation I have made to these numbers is 73.75 to 26.25. Our only methods of analyzing mixtures of these two gasses, (viz. by combustion with a redundancy of oxygen) is not, I believe, sufficiently perfect to afford a nearer coincidence.

The extreme labour and tediousness of the decomposition of ammonia by electricity influenced me, to attempt the discovery of a shorter and more summary method of analysis. The most obvious one was its decomposition by oximuriatic acid gas; but this plan was abandoned, from the impossibility of confining both the gasses by any one fluid; since water acts powerfully on the one, and mercury on the

Attempt at a shorter method of analysis.

* Journal de Physique, 1786, ii, 176.

† Berthollet, jun. lately found the mean of a number of experiments to be 204 from 100. See the following article. C.

other

other. But a mixture of oxygen and ammoniacal gasses more than answered my expectations. When mingled in proper proportions, these gasses, I have ascertained, may be detonated over mercury by an electric spark; exactly like a mixture of vital and inflammable air; and the results of the process, with due attention to the circumstances, which will soon be stated, afford an easy and precise method of analyzing, in the space of a few minutes, considerable quantities of the volatile alkali. With a greater proportion of pure oxygen gas* to ammonia than that of three to one, or of ammonia to oxygen than that of three to 1.4, the mixture ceases to be combustible. When the proportions best adapted to inflammation are used, oxygen gas may be diluted with six times its bulk of atmospherical air, without losing its property of burning ammonia.

A mixture of ammoniacal and oxygen gas detonated over mercury.

Atmospherical air alone does not, however, inflame with ammonia, in any proportion that I have yet tried; though, by long continued electrization with air, ammonia is at length decomposed; its hydrogen uniting with the oxygen of the air and forming water, while the nitrogen of both composes a permanent residuum. Forty-five measures of ammonia being electrified with eighty-six of common air, the total 131 became 136, and 132 after being washed with water. Of 17.2 measures of oxygen, contained in the 86 measures of air at the outset, only 2.9 were left; and these also would probably have disappeared by continuing the operation. If a mixture of ammonia and atmospheric air, each previously dried by caustic potash, and then electrified, be examined, the production of water is made sufficiently apparent on applying ether to the containing vessel. In subjecting ammonia, therefore, to this test of the generation of water by electricity, the purity of the gas from atmospheric air should be carefully determined †.

Atmospherical air does not detonate with ammonia, but decomposes it by long electrification.

* Containing only three or four per cent nitrogen gas.

† The result of this experiment shows, moreover, that, even supposing oxygen to be a constituent of ammonia, we are not to expect its evolution, in a separate form, by electricity; since, when electrified with ammoniacal gas, oxygen gas is deprived of its elastic form, and its base is condensed into water, by union with nascent hydrogen evolved from the alkali.

The

Products of the combustion of ammonia with oxygen vary according to the proportion of the gasses.

The products of the combustion of ammonia with oxygen vary essentially, according to the proportion of the gasses which are employed. If the oxygen gas exceed considerably the ammonia (that is, if its volume be double or upwards) the ammonia entirely disappears; and no gasses remain, but a mixture of nitrogen with the redundant oxygen. The moment the detonation is completed, a dense cloud appears *, and soon afterward settles into a white incrustation on the inner surface of the tube. The quantity of this substance, which is produced, is too minute for analysis; but its characters resemble those of nitrate of ammonia, the acid ingredient of which is probably generated by the action of oxygen on the nitrogen of one part of the volatile alkali. Accordingly, when the excess of oxygen is removed by sulphuret of lime, the nitrogen generally falls short of the proportion, which ought to accrue from a given weight of ammonia; and hence it is scarcely possible to attain, when a considerable excess of oxygen is used, an accurate analysis of the volatile alkali.

When, on the contrary, the ammonia exceeds considerably the oxygen gas, no production of nitrous acid appears to take place; for the residue, after detonation, is quite free from cloudiness. It is remarkable, however, that ammonia, when fired, in certain proportions, with less oxygen than is required to saturate its combustible ingredient, is nevertheless completely decomposed. Part of its hydrogen is sufficient for the saturation of the oxygen; and the remaining hydrogen, and the whole nitrogen of the ammonia, together with that existing as an impurity in the oxygen employed, remain in a gaseous state, and compose a mixture, which may be inflamed by adding a second quantity of oxygen gas, and passing an electric

* In some cases I have observed, that, when the cloud does not occur immediately, it may be made to appear by agitating the quicksilver contained in the detonating tube. This is probably owing to the disengagement of some ammonia, which had lodged with the mercury. The fact confirms what I have already suggested respecting the cause of the variable proportion of gasses, evolved from ammonia by electricity.

spark.

spark*. In this way all the hydrogen of the volatile alkali may be saturated with oxygen, and condensed into water; and the whole of the nitrogen may be obtained as a final result of the process. After determining the amount of the oxygen, consumed both in the first and second combustions, it is easy to calculate the quantity of hydrogen, in the saturation of which it has been employed; for when no nitrous acid has been formed, the hydrogen will be, pretty exactly, double in volume the oxygen which has been expended.

These general observations will tend to render the following experiments more intelligible. They may be divided into two classes, 1st, those in which ammonia was fired with an excessive proportion of oxygen; and 2dly, those in which the oxygen, used in the first combustion, was insufficient, or barely adequate, to saturate the whole hydrogen of the alkali.

I. *Decomposition of Ammonia by an Excess of oxygen Gas.*

Twenty-two measures and a third of ammonia were mixed with $44\frac{2}{3}$ oxygen containing 43 of pure gas. The total Ammonia decomposed by excess of oxygen. became 67 when exploded. Water did not produce any farther diminution, but sulphuret of lime left only 8 measures. Now $34 - 8 = 26$ show the quantity of oxygen gas, which escaped condensation; and this, deducted from the original quantity (43) gives 17 measures for the amount of the oxygen expended. The last number 17, being multiplied by 2, gives 34 for the hydrogen apparently consumed. The final residue $8 - 1.66$ (the nitrogen introduced by the oxygen gas) $= 6.34$ is the nitrogen obtained from $22\frac{1}{3}$ of ammonia; and if to this the hydrogen be added, 40.34 measures of permanent gas will be the total result. Hence 100 measures of the gas producible from ammonia should contain 84.29 hydrogen and 15.71 nitrogen; num-

* This is analogous to what happens, when ether, alcohol, or any of the æriform compounds of carbon and hydrogen, are exploded with a deficient proportion of oxygen; for much of the hydrogen is found in the residuum in the state of gas, and again becomes susceptible of combustion after the addition of a second quantity of oxygen. (*See Mr. Cruikshank's excellent papers in the 5th Volume of Nicholson's Journal, 4to.*)

bers

bers too remote from those, which have been already assigned, to be considered even as approximations to the truth. The error arises from the combination of oxygen, during combustion, not only with the hydrogen, but with the nitrogen of the alkali, the latter of which consequently appears deficient, and the former proportionably in excess.

Proportion of
nitrogen
defective.

Frequent repetitions of this combustion, with a considerable excess of oxygen gas, continued to give a deficient proportion of nitrogen; and as no accurate conclusions can be drawn from experiments of this kind, I shall proceed to those of the second class.

II. *Experiments, in which Ammonia was fired with a deficient Proportion of oxygen Gas.*

Ammonia de-
composed with
a deficiency of
oxygen.

Sixty-three measures of ammonia were exploded over mercury with 33 of oxygen gas containing one of nitrogen. The total 96, when fired by an electric spark, were diminished to 57 measures, which were not contracted any farther by successive agitation with water, and with sulphuret of lime. The whole of the ammonia, therefore, was decomposed; and all the oxygen had entered into combination with the hydrogen of the alkali. The residuary 57 measures were mingled with 40 measures of the same oxygen gas, and detonated by an electric spark; after which the total, 97, were reduced to 60. The diminution, therefore, was 37 measures; and as two thirds of this number may be ascribed to the condensation of hydrogen gas, the residuary 57 must have been composed of 24·66 hydrogen, and 32·34 nitrogen. The oxygen expended, also, was 32 in the first combustion, + 12·33 in the second = 44·33; and this number, being doubled, gives 88·66 for the whole hydrogen saturated, supposing it to be in the state of hydrogen gas. But from the above quantity of nitrogen (32·34 measures) we are to deduct one measure, with which the 33 measures of oxygen were contaminated; and the remainder 31·34 shows the number of measures of nitrogen, resulting from 63 measures of ammonia. The total amount of gasses obtained is 31·34 + 88·66 = 120; and the proportion of the hydrogen by volume to that of the nitrogen, as 73·88 to 26·12.

To

To avoid the tediousness of similar details, I shall state, in the form of a table, the results of a few experiments out of a number of others, all of which had, as nearly as could be expected, the same tendency. The sixth experiment in the table is the one which has been just described.

No. of Ex.	Meas. of Ammon. decomposed.	Meas. of Oxygen saturated.	Meas. of Hydrogen estimated.	Meas. of Nitrogen obtained.	Hence 100 Meas. of Ammonia		Permanent Gas contains in 100 Measures.	
					Take Oxygen.	Give Gas.	Hidr.	Nitr.
1	72	47.5	95	37	66	183	72	28
2	95	64	128	46	67.5	183.3	73.5	26.5
3	100	72.2	144.4	54	72.2	198.5	72.8	28.2
4	74	51.7	103.4	37	69.8	189	73.6	27.4
5	49	33.7	67.4	25.3	68.7	180.2	72.7	27.3
6	63	44.3	86.6	31.3	70.3	193.5	73.9	26.1

Tabulated results.

From an attentive examination of the foregoing table, it will appear, that the results are not perfectly uniform, though perhaps as much as can be expected from the nature of the experiments. Thus the proportion of permanent gasses to the ammonia decomposed (the nitrogen being actually measured, and the hydrogen estimated by doubling the oxygen expended) may be observed to differ considerably

ably; the highest product being $198\frac{1}{2}$, and the lowest $180\cdot2$, from 100 of ammonia. There can scarcely be a doubt, however, that this want of coincidence is owing to the same cause, as that which I have already assigned for the variable proportions of permanent gas, which are obtained from equal quantities of ammonia by electrization. And, accordingly, I have found, that the evolved gasses, as ascertained by combustion, bear the smallest proportion to the ammonia, when most pains have been taken to obviate the presence of moisture. The lowest number, therefore, is to be assumed as most correct; but other circumstances being considered, I believe the second experiment furnishes the most accurate data for determining the composition of ammonia. The same explanation will apply to the different proportions of oxygen gas required for the saturation of 100 measures of ammonia, the variation no doubt arising from the uncertainty of the quantity of alkaline gas which is actually burned. The proportion of oxygen to ammonia, which I believe to be nearest the truth, and most precisely necessary for mutual saturation, is that resulting from the second experiment, *viz.* $67\frac{1}{2}$ measures of oxygen gas to 100 of ammonia, or 100 of the former to 148 of the latter.

Proportions of
hydrogen and
nitrogen.

It may be observed, also, by comparing the numbers in the last two columns of the table, that the hydrogen and nitrogen gasses do not uniformly bear the same proportions to each other. Notwithstanding all the labour I have bestowed on the subject, I have not been able to obtain a nearer correspondence, owing most probably to the imperfection of the mode of analysing a mixture of hydrogen and nitrogen gasses. In the mixture of permanent gasses, determined in this way, the hydrogen, it may be remarked, bears generally rather a less ratio than that of 74 to 26. I do not, however, consider this fact as contradicting the accuracy of the proportions which you have assigned; and it appears to me, that a sufficient reason may be given for the want of a more perfect coincidence between results, obtained by such different methods of investigation. In the products of the electrization of ammonia, the hydrogen composes nearly three fourths of the mixture: and hence
its

its combustion by oxygen gas is likely to be completely effected, and the whole of the hydrogen condensed into water. But after the *partial* combustion of ammonia by oxygen gas, a residuum is left of hydrogen and nitrogen gasses, of which the hydrogen usually composes less, and sometimes considerably less, than one half the bulk. In this case it may be suspected, that a small quantity of hydrogen occasionally escapes being burned; and whenever this happens, its proportion to the nitrogen will appear to be less than the true one*.

From the inflammability of a mixture of ammonia with oxygen gas, it was natural to expect, that this alkali would prove susceptible of *slow* combustion. By means of a peculiar apparatus (on a plan which I have described in the Philosophical Transactions for 1808, part II †, but on a smaller scale, and with the substitution of mercury for water), I have found that ammonia, expelled from the orifice of a small steel burner, may be kindled by electricity in a vessel of oxygen gas; and that it is slowly consumed with a pale yellow flame. The combustion, however, is not sufficiently vivid to render the process of any use in the analysis of ammonia.

With nitrous oxide (containing only 5 per cent impurity) ammonia forms a mixture which is extremely combustible. If the nitrous oxide be in excess, the proportions have a considerable range; for any mixture may be fired by electricity, of which the ammonia is not less than one sixth of the whole. The combustion is followed by a dense cloud, sometimes of an orange colour. When the nitrous oxide greatly exceeds the ammonia, (as in the proportion, for example, of 100 to 30) there is little or no diminution after firing: and the residuum is composed of a small por-

Ammonia susceptible of slow combustion with oxygen.

Mixture of ammonia and nitrous oxide extremely combustible.

* This consideration suggests the propriety of using no more oxygen in the first combustion of ammonia, than is barely sufficient to inflame it; or if a larger quantity has been used than is required for this purpose, and a residue consequently obtained, of which the hydrogen forms only a small proportion, it is proper to add a farther quantity of hydrogen, before the second combustion. An allowance may afterward be made for this addition.

† See Journal, vol. xxii, p. 83.

VOL. XXIV.—SUPPLEMENT.

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tion of undecomposed oxide, some oxygen gas, and a considerable quantity of nitrogen, the last of which, however, is not in its full proportion. When the nitrous oxide is farther increased, still more oxygen is found in the residuum.

When, on the contrary, the alkaline gas is redundant, combustion does not take place, unless the nitrous oxide forms one third of the mixture. A little diminution takes place on firing, but no cloudiness is observed; and the residue is composed of hydrogen and nitrogen gasses, with occasionally a small portion of undecomposed ammonia. As an example of what takes place, I select the following experiment from several others.

Results of an experiment.

A mixture of 41 measures of ammonia with 40 of nitrous oxide (= 38 pure), in all 81 measures, were reduced by combustion to 75, which were found to consist of 16

Explanation of it.

hydrogen and 59 nitrogen gasses. To explain this experiment, we may assume (as is consistent with your own analysis*) that 100 measures of nitrous oxide are equivalent to 52 measures of oxygen gas and 103 of nitrogen. The oxygen in 38 measures of nitrous oxide will, therefore be 19.7, to which, when the oxygen spent in burning the residuum (viz. 8 m.) is added, we obtain 27.7 for the total oxygen consumed; and multiplying by 2, we have 55.4 for the hydrogen saturated. From the residuary nitrogen (59) deduct 39 measures arising from the decomposition of the nitrous oxide + 2 m. mingled with it as an impurity = 41, and the remainder, 18 measures, is the nitrogen resulting from the volatile alkali; and as 41 measures of ammonia give $55.4 + 18 = 73.4$ measures of permanent gas, 100 would give 179 measures, in which the hydrogen and nitrogen would exist in the proportion of 75.4 to 24.6. From the same facts it may be deduced, that 100 measures of ammonia require for saturation 130 of nitrous oxide = $67\frac{1}{2}$ oxygen gas. The coincidence then, between the results of the combustion of ammonia with nitrous oxide, and those with oxygen gas, confirms the accuracy of both methods of analysis.

Confirms the former analysis.

* Researches, Res. ii, Div. 1, or Thomson's System of Chemistry, 3d. edit. ii, 143.

Nitrous gas, which, it appears from your testimony*, does not compose an inflammable mixture with hydrogen, (nor, as I am assured by Mr. Dalton, with any of the varieties of carburetted hydrogen) may be employed, I find, for the combustion of ammonia. The proportions required for mutual saturation are about 120 measures of nitrous gas to 100 of ammonia. An excess of the former gas does not give accurate results; since not only the hydrogen of the ammonia, but some of its nitrogen is also condensed; and the mixture, after being fired, exhibits the cloudy appearance usual in that case.

Forty-eight measures of ammonia, being fired with 60 nitrous gas, (= 53 pure) both gasses were completely decomposed; and a residue left consisting of 61 nitrogen and 9 hydrogen. Sixty measures of ammonia and 41 nitrous gas (= 36.1 pure) gave, after firing, a mixture composed of 10 ammonia, $53\frac{1}{2}$ nitrogen, and $30\frac{1}{2}$ hydrogen. But taking for granted that 100 measures of nitrous gas, according to your analysis, hold in combination a quantity of oxygen equal to $57\frac{1}{2}$ measures of oxygen gas, and of nitrogen equal to $48\frac{1}{2}$ measures; and assuming the proportions of the nitrogen and hydrogen in ammonia, to be those established by your experiments and my own: it will appear from an easy calculation, that the proportion of nitrogen, in the above residua, a little exceeds, and that of the hydrogen rather falls short of what might have been expected. I have not yet been able to reconcile these differences by the numerous trials required in a process of so much delicacy; and I reserve the inquiry for a season of more leisure. The foregoing statement I wish to be considered as merely announcing the general fact of the combustibility of a mixture of ammonia and nitrous gas, a property which chiefly derives importance from its being capable of application to a new method of analysing the latter.

Before concluding this letter, I shall briefly state the results of some experiments, which I have lately made in conjunction with Mr. Dalton, on a subject that formerly oc-

* Researches, p. 136.

bon and hidrogen. occupied much of my attention; viz. the effect of electricity on the aëriform compounds of carbon and hydrogen. Subsequent reflection, as well as the candid and judicious criticisms of various writers *, have influenced me to doubt of the accuracy of a few of the conclusions drawn from my former inquiries †. The knowledge of this class of bodies has, also, been so materially advanced during the last twelve years, that the examination of their properties may now be undertaken with much greater confidence of success than formerly. It is to be lamented, indeed, that experimentalists do not oftener retrace their labours, with the combined advantages of acquired skill, and of a more improved state of the science which they investigate.

Gasses experimented on.

The gasses, submitted by Mr. Dalton and myself to the action of long continued electrization, were carburetted hydrogen from pit-coal of the specific gravity of about 650 (air being 1000), olefiant gas, and carbonic oxide. Each gas was used in as pure a state as possible; muriate of lime being first introduced into the same tubes in which the gasses were electrified, and being withdrawn when it had exerted its full action. Platina wires were used to convey the electric discharges.

Carburetted hydrogen and olefiant gas.

When the electrization of carburetted hydrogen or olefiant gas was continued sufficiently long, they were each found to expand, notwithstanding their extreme dryness. No carbonic acid could be discovered in the electrified gas by the nicest tests. When fired with oxygen, it gave less carbonic acid than the unexpanded gas, and required less oxygen for saturation. Calculating, from the diminished product of carbonic acid, how much gas had been decomposed by electrization, it appeared that the decomposed part, in all cases, was about doubled. The smaller product of carbonic acid from the electrified gas was sufficiently explained by a depo-

* See Berthollet's Chemical Statics, Eng. trans., Vol. II, p. 454; Murray's Elements of Chemistry, Vol. II, Note G; a letter from an anonymous correspondent in Nicholson's Journal, 8vo, II, 185; and Aikin's Dictionary of Chemistry, I, 251.

† "Experiments on Carbonated Hydrogen Gas, with a View to determine whether Carbon be a simple or a compound body." Phil. Trans. Vol. LXXVII.

sition of charcoal on the inner surface of the glass tube, too distinct to be at all equivocal, and most abundant from the olefiant gas. No addition whatsoever of nitrogen was made by the electrization. It appears, therefore, that the hydro-carburetted gasses, like ammonia, are separated by electrization into their element, the carbon being precipitated, and the hydrogen evolved in a separate form, and acquiring a state of greater expansion. This change, however, is effected much more slowly, than the disunion of the elements of ammonia.

From a portion of carbonic acid gas, carefully dried by ^{Carbonic acid} muriate of lime, and electrized with platina conductors, we ^{gas.} obtained, after removing the undecomposed gas by caustic potash, a residuum equal to about one twentieth of the whole gas which had been employed. It was found on analysis to consist of oxygen and carbonic oxide gasses, in such proportions as to inflame on passing an electric spark through it without any addition, and to be thus convertible again into carbonic acid. In the experiments of Mr. Sausure, jun. *, that ingenious philosopher obtained only carbonic oxide by the same operation, owing doubtless to the electricity having been conveyed by conductors of copper, which would become oxidized, and prevent the oxygen from being evolved in a separate form.

Carbonic oxide, electrified with similar precautions, did ^{Carbonic oxide} not appear to undergo any change. Eleven hundred dis- ^{not decom-} charges from a Leyden jar had no effect on a quantity of ^{posed.} the gas, equal to about one tenth of a cubic inch. Its bulk, after this process, was unaltered; no carbonic acid could be discovered in it; and there was no decided trace of oxygen gas in the residuum. The carbon, it appears, therefore, which exists in carbonic oxide, must be held combined by an extremely strong affinity.

With sincere esteem and respect, I am,

Dear Sir,

Your faithful and obliged friend,

WM. HENRY.

* Journal de Physique, Tom. LIV, p. 450.

V.

*Observations on the Composition of Ammonia; by
Mr. BERTHOLLET, jun.**

Oxygen sought
for in ammonia.

THE object of Mr. Berthollet was to search for the oxygen, which, according to Mr. Davy, ammonia contains in the proportion of 20 per cent †.

Expansion of
ammoniacal gas
by electrifica-
tion.

He repeated by more direct means the analysis made by Mr. Davy. He ascertained the expansion of ammoniacal gas, when, from the effect of electric shocks repeated for a long time, its elements have resumed their natural elasticity. The analysis of the gaseous mixture resulting from this operation afterward showed the nature and proportion of the substances composing it. The mean of a great number of experiments indicates, that, when ammonia is decomposed by the electric fluid, its bulk increases in the ratio of 100 to 204‡; and that the gas thus formed consists of 755 hydrogen, and 245 azot. Calculating from the refractive power of the gasses, Mr. Berthollet estimates, that 775 parts of ammonia by weight, produced 776 of the two gasses; and that their proportions by weight were, hydrogen 18·87, azot 81·13.

and no oxygen,
unless contain-
ed in these.

The following is his inference from his experiments. Ammonia is composed of hydrogen and azot, and no oxygen can be found in it, unless by some process yet unknown we should be able to extract oxygen from gasses, that have always been considered as pure azot and nitrogen §.

Ammonia de-
composed by
heat in a porce-
lain tube.

The gas collected by decomposing ammonia in a red hot tube of porcelain contains the same proportions of hydro-

* *Annales de Chimie*. August, 1808, vol. lxxvii, p. 218.

† "Seven or eight, at least; possibly more." See *Phil. Trans.* for 1808, p. 40; or *Journal*, vol. xx, p. 329. C.

‡ This exceeds all Dr. Henry's proportions of increase except one. Mr. B. too makes the proportion of hydrogen greater than either Dr. Henry, or Mr. Davy. See the preceding article. C.

§ It would appear by Mr. Davy's experiments, see *Journal*, vol. xxiii, p. 242. &c. that nitrogen is an oxide, if not hydrogen likewise; and these will be farther confirmed in that gentleman's appendix to his former paper, which will be given next month. C.

gen and azot as the preceding. In an experiment of this kind, when twenty quarts of ammoniacal gas were decomposed with every precaution for condensing the water, that should have been formed if ammonia contained $\frac{1}{3}$ of oxygen, none was obtained.

The decomposition by the electric spark shows no trace of humidity, or of oxidation, when an iron wire is employed; yet one or other of these effects must inevitably take place, if there were any oxygen in ammonia.

No oxygen in the decomposition by electricity.

VI.

Analysis of the Chinese Rice-Stone, with some Observations on the Yu; by Mr. KLAPROTH.*

THE rice-stone, of which the Chinese make cups, and other vessels, which are occasionally brought to Europe, is an artificial production, the component parts of which are yet unknown. Authors are by no means agreed on the origin of its name. Storr informs us, that many collectors of curiosities in Holland assured him it was actually prepared from rice, which was hardened by the addition of other substances. Bruckmann on the other hand supposes, that it received its name from its resemblance to transparent rice. It has also been considered by some as alabaster: by others as chalcedony, or one of its varieties, cacholong; and lastly as the problematic stone, or yu, which will be mentioned hereafter. Mr. Kratzenstein, of Copenhagen, has at length ascertained what this substance is, and gives the following description of a cup.

Rice-stone an artificial product,

said to be prepared from rice.

This substance is a fusible glass, resembling in colour a white jelly. It is formed in a mould of two pieces, while it is soft. It is ornamented with figures and handles in relief. The sharp edge occasioned by the mould was still observable. It is so hard as to scratch glass, and is much more difficult to cut than marble. Its fracture has a dull lustre, like that of starch jelly dried. Its colour and transparency much resemble alabaster,

A fusible glass.

* *Annales de Chinie*, vol. lxxix, 302. From *Gehlen's Journal*.

Examined by
Crell.

Crell subjected the rice-stone to several chemical experiments in 1781, to find whether it contained rice or its mucilage. He exposed some pieces to a strong red heat in a small crucible, but found no indication of any volatile matter, animal or vegetable. The pieces were agglutinated together, and adhered to the bottom of the retort: and the substance still retained its colour and semitransparency, as before the experiment, and had undergone no loss.

Analysed.

As no farther experiments on the component parts of the rice-stone have been published, its nature has still remained unknown. I have attempted an analysis of it on a very small quantity; and though this analysis is not very exact, it is sufficient to throw some light on the composition of the stone. The portion analysed was taken from a vessel with two handles, weighing 12 ounces. From its external appearance it might be taken for a greenish gray chalcedony, as well on account of its polish and transparency as of its colour: but the sound it emitted when struck, and still more its specific gravity, which was above double that of chalcedony, since it was 5.3936, showed unquestionably, that it was not this stone.

Specific gravity
of a specimen.

Hardness.

Treated before
the blow-pipe.

It is easily attacked by the file. It breaks easily, with a conchoid fracture, and glassy lustre. In a small spoon before the blowpipe it readily fuses into a small bead; but on a piece of charcoal this bead is covered with a leaden gray pellicle. Borax and the phosphoric salts difficultly combine with it: but if it be fused in the small platina spoon with carbonate of soda, immediately small globules of metallic lead appear. Acids do not act on this stone; accordingly I attacked it by alkalis in the following manner.

Silex.

a. One hundred grains of this stone reduced to an impalpable powder were heated red hot with potash. The mixture became hard, and acquired an ashen gray colour. On supersaturating it with nitric acid silex was separated to the weight of 39 grains.

Oxide of lead.

b. Sulphate of soda being added to the solution, sulphate of lead was precipitated, weighing 55 grains, which indicate 41 grains of oxide of lead.

c. The

c. The liquor separated from the sulphate of lead, being Alumine. mixed with ammonia, yielded 7 grains of alumine.

An addition of carbonate of ammonia produced no farther alteration.

Thus 100 parts of the rice-stone gave							Its component parts,
Oxide of lead	-	-	-	-	-	41	
Silex	-	-	-	-	-	39	
Alumine	-	-	-	-	-	7	
<hr/>							
87							

It may fairly be presumed, that the 13 parts deficient were some vitrifying principle, either borax, soda, or potash: but the small quantity sacrificed for this analysis did not allow a repetition of experiments. Some saline flux.

From the results of this analysis it appears, that the pretended stone or paste of rice is nothing but a siliceous glass of lead, to which a resemblance of chalcedony is given by means of alumine. But it is not necessary, to employ alumine purified by art for the preparation of this glass. It is even very probable, that the Chinese employ feldspar, or petuntze, with the properties of which they are well acquainted, since it is with this substance and kaolin, that they make their porcelain. Its fabrication.

Preliminary experiments have shown me, that a substance similar to the rice-stone may be prepared, by fusing together 8 parts of oxide of lead, 7 of feldspar, 4 of common white glass, and 1 of borax: or by employing 8 parts of oxide of lead, 6 of feldspar, 3 of silex, and 3 of borax, potash, or soda. Confirmed by experiments.

It appears, however, that no determinate proportions of oxide of lead are observed in the preparation of the rice-stone. Hence the specific gravity of this stone varies considerably, so that several other stones which I tried, or which have been examined by others, were near a third lighter than the stone I analysed. The specific gravity of one small cup resembling the former, but ornamented with antique Chinese figures, I found to be 3.68; that of several fragments of a thin vase, 3.635; that of a drop for the ear, in the shape of a long pearl, and labelled "oriental nephritic stone", 3.58. Crell found the specific gravity of a rice- Variable in the proportion of ingredients.
Different specific gravities.

a rice-stone vase, preserved in the collection of natural history at Brunswick, 3.768; that of another small cup, 3.5; and that of the pieces he made his experiments on, 3.75.

The *yu* perhaps
artificial.

As to the *yu* it is known only from the memoirs of the *Pekin Missionaries*. It is surprising, that a stone so much vaunted on account of its beauty, hardness, and the sound it emits when struck, should be unknown in Europe. Mr. Hager has given a description of a vase preserved at Paris, which he supposes to be made of this stone: but its being so is very doubtful; and we may even presume from his description, that it is merely an artificial production analogous to the rice-stone. The missionaries indeed speak of it as a natural production: but the sonorous property of the stone leads to the conjecture, that it is nothing but a vitreous composition. Though we know several sonorous stones, as the *klingsstein*, or *perphir-schiefer*, and the sonorous quartz crystals of Prieborn, the sounds they emit are not comparable to those of the *yu*; nor can instruments of music be made of them, as of this stone. We cannot however absolutely deny, that sonorous stones are found in China, of which instruments of music are made: for a proof of this is found in a Chinese *king*, in the collection of Mr. Bertin, at Paris, an analysis of which was published by the duke de Chaulnes, who found it to be a black bituminous marble.

But this by no
means certain.

Sonorous
stones,

known to the
ancients-

Pliny, lib. 37, cap. 10, mentions a black stone, as sonorous as brass, by the name of *chalcophonos*, which was given it because it sounded like brass when struck.

VII.

*On the Effect of westerly Winds in raising the Level of the British Channel. In a Letter to the Right Hon. Sir JOSEPH BANKS, Bart. K. B. P. R. S. By JAMES REE-NELL, Esq. F. R. S. **

DEAR SIR,

IN the "*Observations on a Current that often prevails to the Westward of Scilly*," which I had the honour to lay before the Royal Society many years ago, I slightly mentioned, as connected with the same subject, the effect of strong westerly winds in raising the level of the British Channel; and the escape of the superincumbent waters through the Strait of Dover, into the then lower level of the North Sea.

The recent loss of the Britannia East India ship, Captain Birch, on the Goodwin Sands, has impressed this fact more strongly on my mind; as I have no doubt that her loss was occasioned by a current, produced by the running off of the accumulated waters; a violent gale from the westward then prevailing. The circumstances under which she was lost, were generally these:

In January last she sailed from her anchorage between Dover and the South Foreland (on her way to Portsmouth), and was soon after assailed by a violent gale between the west and south-west. The thick weather preventing a view of the *lights*, the pilot was left entirely to the reckoning and the lead; and when it was concluded, that the ship was quite clear of the Goodwin, she struck on the north-eastern extremity of the southernmost of those sands. And this difference between the reckoning (after due allowance being made for the tides) and the actual position, I conclude was owing to the northerly stream of current, which caught the ship when she drifted to the back, or eastern side of the Goodwin.

The fact of the high level of the Channel, during strong winds between the W. and SW., cannot be doubted: be-

*Philos. Trans. for 1809, p. 400.

cause the increased height of the tides in the southern ports, at such times, is obvious to every discerning eye. Indeed, the form of the upper part of the Channel, in particular, is such as to receive and retain, for a time, the principal part of the water forced in; and as a part of this water is continually escaping by the Strait of Dover, it will produce a current; which must greatly disturb the reckonings of such ships as navigate the Strait, when thick weather prevents the land or the lights of the Forelands, and the North Goodwin, from being seen.

Effects of SW. winds on the tides in the Channel,

I observe in a new publication of Messrs. Lawrie and Whittle, entitled "*Sailing Directions &c. for the British Channel*, 1808," that throughout the Channel, it is admitted by the experienced persons whom he quotes, that strong SW. winds "cause the flood tide to run an hour, or more, longer, than at common times: or in other words, that *a current overcomes the ebb tide a full hour*: not to mention how much it may accelerate the one, and retard the other, during the remainder of the time*.

Direction of the current from the shape of the shores of the Channel.

It is evident, that the direction of the current under consideration will be influenced by the form and position of the opposite shores, at the entrance of the Strait; and as these are materially different, so must the direction of the stream be, within the influence of each side, respectively. For instance, on the English side, the current having taken the direction of the shore, between Dungeness and the South Foreland, will set generally to the north-east, through that side of the Strait. But, on the French side, circumstances must be very different: for the shore of Boulogne, trending almost due north, will give the current a like direction, since it cannot turn sharp round the Point of Grisnez, to the north-eastward; but must preserve a great proportion of its northerly course, until it mixes with the waters of the North Sea. And it may be remarked, that the Britannia, when driven to the eastward of the Goodwin, would fall into this very line of current.

There

* It is also asserted, that in the mouth of the Channel, the extraordinary rise of tide, in stormy weather, is ten feet: that is, at common springs, twenty, and in storms thirty feet. See pages 28, 41, 70, and 133.

There is another circumstance to be taken into the account; which is, that the shore of Boulogne, presenting a direct obstacle to the water impelled by the westerly winds, will occasion a higher level of the sea there than elsewhere; and of course a stronger line of current towards the Goodwin.

It must, therefore, be inferred, that a ship, passing the Strait of Dover, at the back of the Goodwin Sands, during the prevalence of strong W. or SW. winds, will be carried many miles to the northward of her reckoning; and if compelled to depend on it, may be subject to great hazard from the Goodwin. Dangerous consequence of this.

It will be understood, of course, that although the stream of current has been considered here (in order to simplify the subject), yet that, in the application of these remarks, the regular tides must also be taken into the account. The regular tides subject to much the same laws as the current. But from my ignorance of their detail, I can say no more than that I conceive, that the great body of the tide from the Channel must be subject to much the same laws, as the current itself. The opposite tide will doubtless occasion various inflexions of the current, as it blends itself with it; or may absolutely suspend it: and the subject can never be perfectly understood, without a particular attention to the velocity and direction of the tides in moderate weather, to serve as a ground-work*.

I am, with great respect,

Dear Sir,

Your faithful humble Servant,

J. RENNEL.

VIII.

On dead Lime, by MR. BUCHOLZ †.

IT has long ago been said, that under certain circumstances which are not yet well ascertained, and particularly after

* Messrs. Lawrie and Whittle's publication allows the tides in this quarter a velocity of one mile and a half per hour at the springs; half a mile at the neaps. The Britannia's accident happened at *dead neaps*.

† *Journal des Mines*, vol. xxii, p. 234.

a violent

Lime that will a violent and long continued fire, limestone may be converted into a kind of lime that does not heat with water, and does not slack; and this has been called *dead lime*. This state of lime does not appear to be recognized by all chemists, since it is not mentioned in elementary treatises on chemistry: some however think, that clay combined with lime may give it the property of hardening by great heat, and thus cause it to lose those of heating and slacking with water, giving rise to dead lime. Perhaps I may remove the uncertainties, and reconcile the different opinions, to which this substance has given rise.

May be owing to alumine, Four cases may be supposed, to each of which lime may pass to this state.

1. When it contains a great deal of clay, and is heated so strongly as to become very hard. In this state it will not effervesce with acids, because all the carbonic acid is expelled.

2. If it contain silex, and be heated strongly after the complete expulsion of the carbonic acid, it will not effervesce with acids.

3. In the third case, lime being heated at once very violently, forms a mass perfectly similar to dead lime. It passes into a semifluid state, the possibility of which I have shown in the Berlin Journal, and it requires to be heated gradually, to expel the carbonic acid of large pieces in particular. When the kiln is emptied, there will remain pieces half-fused, that will neither heat nor slack with water, but effervesce with acids: these are carbonate of lime, fused or hardened by the fire.

4. Lastly on calcining carbonate of lime in a fire continued long after the expulsion of the carbonic acid, a true dead lime is formed, which neither heats with water, nor effervesces with acids. All the circumstances of its formation are not yet well known.

I saw some years ago this kind of lime formed by the calcination of chalk and of oyster-shells; but not being satisfied, that they contained neither silex nor alumine, I ascribed to these earths the peculiar properties of the lime obtained. Lately the same phenomena occurred to me, and as I was very certain, that the oyster-shells employed contained no earth but lime, no phosphate of lime, and no salt soluble in water,

neither heat
nor slack.

May be owing
to alumine,

silex,

too hasty and
strong a fire,

or fire too long
continued.

Dead Lime
from oyster-
shells.

water, I can affirm, that the properties of the lime obtained were altogether independent on the presence of these circumstances: yet I obtained from the same shells, by a somewhat more gentle heat, common caustic lime easily slacked.

The dead lime obtained heated very strongly with muriatic its properties. acid diluted with a small quantity of water, without emitting the smallest bubble of carbonic acid. Pieces of it remained in water for twenty-four hours without falling to powder; and notwithstanding this common lime-water was formed, which is very remarkable. When the calcined oyster-shells were thrown into a boiling lixivium of carbonate of soda, the soda was completely decomposed, and a very fine pap was formed.

If this account be not sufficient to throw much light on the subject in question, it will serve at least to guide the reflections of men of science, and show how different opinions on the existence of dead lime may be reconciled.

IX.

On the Muriates of Barytes and of Silver; by BERTHIER, Mine Engineer.*

IN a paper on the sulphate of barytes &c. + I took it for granted, that muriate of silver contained 20 per cent of Proportion of muriate of silver mistaken. muriatic acid, and hence I deduced the composition of the muriate of barytes, which I afterward employed in my inquiries concerning the sulphates. Recent experiments having taught me, that this supposition was not strictly accurate, I endeavoured to ascertain with more precision the proportions of the muriates of barytes and silver.

I put 10 gr. [154 grains] of barytes recently obtained from the calcined nitrate into a glass stopp'd phial filled with water. 0.4 of a gr. of carbonate of barytes remained undissolved. The solution was supersaturated with some muriatic acid, and evaporated to dryness. The residuum, calcined in a platina crucible, weighed 12.75 gr. It contained 3.15 gr. of muriatic acid, since I had employed Muriate of barytes formed.

* *Journal des Mines*, vol. xxii, p. 323.

† See *Journal*, vol. xxiii, p. 280.

9.60 gr. of caustic barytes. Calcined muriate of barytes therefore is composed of

Its component parts, when calcined,	barytes	0.753	muriatic acid	0.247
	I formed anew some muriate of barytes from its component principles, and crystallized it. 10 gr. lost 1.5 gr. by calcination, consequently the crystallized salt contains			
when crystallized.	barytes	-	-	0.64
	muriatic acid	-	-	0.21
	water	-	-	0.15
				<hr/> 1.00

Muriate of silver. Five gr. of artificial muriate of barytes calcined, containing 1.235 gr. of acid, were precipitated by nitrate of silver; and the result was 6.75 gr. of muriate of silver. This salt therefore contained

muriatic acid . . . 0.183, and as it contained
silver - - - 0.750, there remain
0.067, which must represent the oxygen.

1.000

Proust's proportions Mr. Proust has found, that 100 parts of silver always produce 133 of muriate; and on precipitating a salt of silver by lime-water, he found, that the precipitate was composed of 0.905 silver, 0.01 lime, and 0.085 oxygen.

for oxide of silver, Hence he concluded, that the oxide of silver contained

silver	-	-	-	0.909
oxygen	-	-	-	0.091
				<hr/> 1.000

and muriate of silver, and the muriate of silver,

silver	-	-	-	0.751
acid	-	-	-	0.180
oxygen	-	-	-	0.069
				<hr/> 1.000

Sulphate of barytes. It is difficult in chemistry to obtain results agreeing more nicely. 8.5 gr. of calcined muriate of barytes, which I had prepared, were precipitated by sulphate of soda, and produced 9.55 gr. of sulphate of barytes, a quantity differing very little from what I had before found; and whence it followed, that the sulphate contained 0.345 of acid*. The proportions of the muriate, which I have just ascertained, prove, that the sulphate of barytes cannot contain more than 0.33 of acid.

* This was the largest proportions shown by Mr. Berthier's analyses. C

I N D E X.

A.

ACID, boracic, decomposition of, 12, 24

—fluoric, Inquiries respecting, 20, 29

—acetic, obtained from wood, 70

—muriatic, analytical experiments on, 95

—artificial succinic, 319

Acton, Mr. J. on respiration, 130, 307

Aërial navigation, 164

Albumen, a substitute for Peruvian bark in intermittent fevers, 78

Aleman, M. on the analysis of an urinary stone, 317

Alkalies, how affected by phosphorus, 285

Alkaline metals, *see* Metals

Alumine in meteoric stones, 190

Ammonia, new method of analysing, 358

—its composition, 374

Analogy of structure in animals, 72

Analysis of onions, 290—Of an urinary stone, 317—Of the deadly nightshade, 317—Of the rice-stone of the Chinese, 375

Analytical experiments on the decomposition and composition of boracic acid, 12—inquiries respecting fluoric acid, 20—of muriatic acid, 95

Anchors, improvement in, 46

Animal structure, 72

Astringents, vegetable, 204, 241

Atmospheric phenomena, 106

B.

Bakerian Lecture (concluded from Vol. XXIII.) 12, 95

Ball, Capt. H. L. his improvement in the construction of anchors, to render them more durable and safe for ships, with an improved mode of fishing anchors, 46

Banks, Mr. letter from, respecting the Meteorological Journal, 308

Barlow, Mr. W. his improved screw wrench, to fit different sized nuts or heads of screws, 61—His demonstration of the Cotesian theorem, 278

Barometer, correction of the height of, 81

Barometrical measurement of heights, 63

Bate, Mr. R. B. on the camera lucida, 146

Bayen, M. 190

Bernoulli on sound, 310

Berthier, M. on the muriates of barytes and silver, 383

Berthollet, jun. M. on fluoric acid, 31
—On the composition of ammonia, 374

Bertin, Mr. his analysis of the Chinese king, 378

Bessel, M. on the comet of 1807, 198

Bicket's Theory of Respiration, 140, 307

Biggin, Mr. his experiments to ascertain the proportion of tan in different barks, 2

Blumenbach, M. 296

Bolton, Capt. W. his improved method of forming jury-masts, 44

Bones, fossil, 193, 295

Boracic acid, *see* Acid

Bosc, M. on the species of ash 79

Bostock, Dr. on the union of tan and jelly, 1—On vegetable astringents, 204, 241

Botany, present state of, in France, 74

Bouillon-Lagrange, *see* Lagrange

Bourne,

INDEX.

Bournon, Comte de, on the triple sulphuret of lead, copper and antimony, 225, 251, 321
Bradley, Mr. on grafting and budding of trees, 338
Brain, structure of, 72
Bruckmann on the rice-stone of the Chinese, 375
Brochaut, M. on "Transition Strata," 78
Brogniart, A. on the glauberite, 65—
 On the strata in the vicinity of Paris, 77
Broussonnet's theory of the construction of the leaves of the sensitive plant, 268
Bucholz, M. on dead lime, 382
Burney, James, Esq. on the progress of bodies floating in a stream; with an account of some experiments made in the river Thames, with a view to discover a method for ascertaining the direction of currents, 49—His new method of measuring a ship's velocity, 57

C.

C. on the best shape for stills, 204
Cadell, Mr. 13
Cadet, M. his experiments on camphorated water as a test of soda, 319
Camera lucida, 146
Camper, M. 297
Cayley, Sir G. on aerial navigation, 164
Cels, M. 79
Chaptal, M. on the best shape for alembics, 201—His account of some colours for painting, found at Pompeii, 32
Children, J. G. Esq. his account of some experiments performed with a view to ascertain the most advantageous method of constructing a Voltaic apparatus, for the purposes of chemical research, 150
Chladni, M. on the vibrations of elastic surfaces, 312

Clouds, formation of, 106
 Colours for painting, found at Pompeii, 302
 Comets, 197
Cook, Mr. B. on the use of iron for stairs and instead of the timbers of houses, as a security against fire, 126
 Copal varnish, free from colour, 67
Cordier, M. on the dusodile, a new species of mineral, 223
Cork, preparation of, for modelling, 222
Cotesian theorem demonstrated, 278
Cotton, to be cultivated in France, 79
Crell on the Chinese rice-stone, 376
Cruikshank, Mr. his fulminating silver, 237
Cubières, M. De, on the lote-tree, 74
Curaudau, M. on boracic acid, 24—
 His description of a process, by means of which potash and soda may be metallized without the assistance of iron, 37, 285—On the new properties of the alkaline metals, 40—On the influence the shape of a still has on the quality of the product of distillation, 201
 Currents of water, experiments on, 49
 Currents in the British Channel, 379
Cuvier, M. on the skeletons of animals found in the earth, 76—On the strata in the vicinity of Paris, 77—On the fossil bones found in Corsica, 196—
 On the species of carnivorous animals, the bones of which are found mixed with those of bears in caverns in Germany and Hungary, 295

D.

D'Alembert's equation of sound, 310
D'Arcet, M. on the glazing of pottery, 305
D'Arcet, jun. M. on the impossibility of freeing soda and potash from water, 71
Darwin, Dr. on the vegetable life, 262
Daubenton, M. 298

Davy,

INDEX.

Davy, Mr. his experiments on the tan contained in astringent substances, 2
 —His account of some new analytical researches on the nature of certain bodies, 12, 95—On the metallization of soda and potash, 38, 41—On vegetable astringents, 206, 243—On the nature of alkalies, 285
Decandolle, M. on the compound flowers of plants, 74
Degen, M. his mechanical means of aerial navigation, 173
Descotils on the blue colour in some Egyptian hieroglyphics, 304—On detonating silver, 237
Detonating silver, 237
Deyeux, M. on vegetable astringents, 206, 241
Dolomieu, M. 223
Dony, M. his lamination of zinc, 70
Doors for rooms, improvement in, 59
Dree, M. his examination of the experiments of Sir James Hall, 70
Dukamel, M. on grafting, 337
Duméril, Professor, on the analogy of structure in animals, 72
Dusodile, a new species of mineral, 223

E.

Earthenware, improvement in the manufacture of, 305
E. F. G. H. on the application of a series to the correction of the height of the barometer, 81.
Electricity, experiments in, 174, 179, 283
Ellis, Mr. on respiration, 140, 307
Endellion, not a simple ore of antimony, &c. 228, 251, 321
Eringums, 74
Esper, M. 299
Euler's hypothesis of sound, 310

F.

Fecula changed by heat, 319
Figuier, M. on detonating silver, 237
Fire, security against, 126
Fischer, M. 298
Fluoric acid, *see* Acid
Foresyth, Mr. on grafting and budding, 339, 350
Fossil bones, found in America, 76—
 In Corsica, 193—In Germany and Hungary, 295
Fourcroy, M. on animal mucus, and uree, 72—On meteoric stones, 190—
 On the chemical analysis of onions, 290
French National Institute, Transactions of, 68, 309
Fruit-trees, cultivation of, 158

G.

Gall, Dr. on the structure of the brain and nervous system, 72
Galvanic experiments, 179
Gardom, Mr. 267
Gay-Lussac, M. on boracic acid, 13, 24
 —On fluoric acid, 29—On the conversion of soda and potash into metals, 38, 285—On the proportion of metal and oxygen in metallic salts, 71
 —On the action of the metal of potash on metallic salts and oxides, and on alkaline and earthy salts, 92
Gelatine, used successfully in the cure of intermitting fevers, 78
Gerard, M. his drawings of the alterations of solar light, 157
Girod-Chantrons, M. on the natural history of the department of the Doubs, 78
Glauberite, a new metal, 65
Gmelin on vegetable irritability, 262
Grafting and budding of trees, 337, 346
Grasses, germination of, 73
Grape sugar, 71
Grew, M. 337

INDEX.

H.

- Hager, M. 378
 Hail, formation of, 110
 Hall, Sir James, his experiments on metals exposed to great heat under pressure, 70
 Haller on the motion of plants, 261
 Hassenfratz, M. on the alterations that the light of the sun undergoes in traversing the atmosphere, 155
 Hatchett, Mr. his artificial tan, 5, 317
 Haüy, M. on the alterations of solar light, 155, 223
 Hazard, M. 79
 Heights measured by means of the barometer, 63
 Henry, Dr. his experiments on ammonia, and an account of a new method of analysing it, by combustion with oxygen and other gases, 358
 Howard, Mr. his fulminating silver, 237

I.

- Ibbetson, Mrs. A. on the cause of motion in plants, and what is called the sleep of plants, 114, 273—On the effects produced by the grafting and budding of trees, 337—On the defects of grafting and budding, 346
 Iron substituted for timbers in buildings, 126
 Irritability of vegetables, 121, 261
 Italian Institute, Transactions of, 314

J.

- Jacquín, Professor, on the vibrations of the various kinds of gas, 316
 Jaume St. Hilaire, M. on the orobanches, 74
 J. B. on the measurement of heights by the barometer, 63
 Jelly, union of, with tan, 1
 J. F. on the introduction of air into the blood through the lungs, 307
 Jury-masts, improved, 44

K.

- King, a sonorous stone, in China, 323
 Klaproth on meteoric stones, 190—His analysis of the Chinese rice-stone, with some observations on the Yu, 375
 Krafzenstein, M. on the rice-stone of the Chinese, 375

L.

- Lagrange on the eccentricities of the planets, 70—On the Cotesian theorem, 278—On the action of phosphorus and oxygenized muriatic acid gas on alkalies, 285
 Lake, process for making, 238
 Lampadius, 319
 Laplace on the constant acceleration of the planets, 69—On the alterations of solar light, 155—On comets, 200
 Lectures at the London Hospitals, 79, 158
 Lenormand, M. his colourless copal varnish, 67
 Lescallier, M. on the climate of Genoa, 78
 Light, theory of, 113
 Light, solar, alterations of, 155
 Lightning, theory of, 109
 Lime, dead, 382
 Link on the anatomy of plants, 73
 Longchamp, M. on narcissuses, 74
 Lote-tree, 74
 Lyall, Mr. R. on the irritability of vegetables, 261

M.

- Macquer, 70
 Malpighi, M. 337
 Margraff, M. 190
 Marum, Van, on the irritability of the vessels of plants, 275
 Masts, *see* Jury-masts.
 Measurement of heights by the barometer, 63

Melandri,

INDEX.

Melandri, Dr. G. on the artificial tannin of Mr. Hatchett, 317
 Metallic salts, *see* Salts.
 Metals, alkaline, new properties of, 40
 Meteoric stones, analysis of, 190
 Meteorological Journal, for August, 80
 —September, 160—October, 240—
 November, 320
 ——— letter concerning, 308
 Meteors during a thunder-storm in August last, 161
 Miller, Mr. 337
 Mirbel mistaken as to the cause of motion in plants, 72, 115, 337
 Mollerat, M. his manufacture of vinegar from wood, 70
 Mons, Van, on atmospheric phenomena, 106—On the cultivation of fruit-trees, 158—On Galvanism and electricity, 179
 Morveau, M. his invention of a pyrometer, 71
 Motion in plants, 114, 261
 Mucus, animal, 72
 Muriatic acid, *see* Acid.

N.

Nervous system, its structure, 72
 Nightshade, analysis of, 317

O.

Olbers, Dr. his discovery of the perturbations of the planet Pallas, 68—On comets, 198
 Olivier, M. on coleopterous insects, 75
 Onions, analysis of, 290
 Oxides, how affected by potassium, 92

P.

Pacchiani's Galvanic experiments, 179
 Pallas (planet) prize for the discovery of a theory of its perturbations, 68
 Paradisi, M. on the vibrations of sound, 314

Parmentier, M. his improvement of grape sugar, 71
 Péron, M. his collection of insects of the Medusa class, 75
 Petit Thouars, M. Du, on the genus orchideæ, 74
 Petrifications, 78
 Phenomena, atmospheric, 106
 Phosphorus, its action on alkalies, 285
 Picot, Professor, on comets, 197
 Planets, anatomy of, 72—Compound flowers of, 74—Motion of, 121, 261
 Poisson on planetary motion, 70
 Pompeii, colours found at, 302
 Poncelet, M. his lamination of zinc, 70
 Potassium, how obtained, 37, 318—Its action on salts and oxides, 92
 Potash and soda, process for metallizing, without the assistance of iron, 37, 318—Cannot be freed from water, 71
 Prize questions, proposed by the French National Institute, 68, 312—By the Russian Imperial Academy, 317
 Proust, M. his extraction of sugar from grapes, 71
 Pyrometer, 71

R.

Rain, *see* Clouds.
 Rampasse, M. on a calcareous breccia containing fossil bones, found in Corsica, 193
 Rennell, J. Esq. on the effect of westerly winds in raising the level of the British Channel, 379
 Repetends, singular property of, 124
 Respiration, experiments on, 130, 307
 Rice-stone of the Chinese, analysis of, 375
 Rickman, Mr. his experiments on the currents of water, 52
 Ritter, M. on the metallic product of potash, 313
 Roche, M. de la, on the monography of eringums, 74

Rosenmueller,

INDEX.

Rosenmueller, M. 296
 Roth on the contraction of the leaves of
 the drosera, 266
 Rudolph on the anatomy of plants, 73
 Russian Imperial Academy, transactions
 of, 317
 R. Z. A. on the preparation of cork for
 modelling, 222

S.

Sage, M. on some petrifications, 78—
 His process to ascertain the existence
 of alumine in meteoric stones, 190
 Sailing, new method proposed for mea-
 suring a ship's velocity of, 57
 Saint, W. Esq. on a curious property
 of single repetends, 124
 Salts, metallic proportion of metal and
 oxygen in, 71—How affected by potas-
 sium, 92
 Sauveur, M. on sound, 309
 Schroeter, M. 198
 Scientific News, 68, 158, 239, 309, 382
 Screw-wrench, an improved, 61
 Seebeck, Dr. 319
 Seguin, M. on albumen as a substitute
 for Peruvian bark, in intermittent
 fevers, 78
 Senebier, on vegetable motion, 262
 Series, application of a, to the correc-
 tion of the height of the barometer,
 81
 Sheldrake, Mr. on the camera lucida,
 146
 Silver, detonating, 237
 Singer, Mr. his electro-chemical experi-
 ments, 174
 Skeletons of animals found in the earth,
 76
 Sleep of plants, 121
 Smith on the irritability of plants, 262,
 271
 Smithsonian, Mr. his criticisms on the
 Count de Bournon's memoir on the
 triple sulphuret of lead, copper, and
 antimony, answered, 225, 251

Soda, *see* Potash.
 Solar light, *see* Light.
 Sound, investigation of, 309
 Spurcheim, Dr. 72
 Staveley, James, Esq. his account of
 some luminous meteors seen during a
 thunder storm, 161
 Stebbing, Mr. 145
 Steinacher, P. A. on the blue wolfs-
 bane, 236
 Stills, shape of, 201
 Storr, on the rice-stone of the Chinese,
 375
 Sue, M. 179
 Sugar of grapes, 71
 Sulphuret, triple, of lead, copper, and
 antimony, 225, 251, 321
 Surrey Institution, lectures at, 239

T.

Tad, Mr. J. his method of preventing
 doors from dragging on carpets, or
 admitting air under them, 59
 Tan and jelly, 1
 Tan, an artificial, 5, 317
 Taylor on sound, 310
 Tellurium, experiments on, 318
 Tessier, M. 79
 Thenard, M. on the decomposition of
 boracic acid, 18, 24—On fluoric acid,
 20, 29—On the conversion of potash
 and soda into metals, 38, 285—On
 the action of the metal of potash on
 metallic salts and oxides, and on al-
 kaline and earthy salts, 92—His pre-
 paration of smalt for oil paint, 304
 Thomson, Dr. his experiments on
 glue, 7
 Thunder storms, formation of, 106
 "Transition Strata" what, 78
 Trees, grafting and budding of, 337,
 346
 Treviranus on the anatomy of plants, 73
 Trommsdorff, M. his artificial succinic
 acid, 319

Vacca,

I N D E X.

V.

Vacca, M. on the influence of electricity on flame, 283
 Van Mons, *see* Mons.
 Van Marum, *see* Marum.
 Varnish, an excellent copal colourless one, 67
 Vauquelin, M. on animal mucus and uree, 72—On meteoric stones, 190
 —On the chemical analysis of onions, 290
 Vegetable astringents, 204, 241
 Vegetable irritability, 121, 261
 Ventenat, M. on a new family of plants, 74
 Villars, M. on the construction of the coats of the nerves, 72
 Vinegar obtained from wood, 70
 Vogel, M. on the action of phosphorus and oxygenized muriatic acid gas on alkalies, 285
 Voltaic apparatus, best construction of, 150

U.

Uree, animal, 72

W.

Werner's "Transition Strata," 78
 Whately, Mr. on the contraction of the leaves of the drosera, 266
 Willdenow, 337
 Winds, their effects on the waters of the British Channel, 379
 Withering, Dr. on the contraction of the leaves of the drosera, 266
 W. N. on destroying the elasticity of cork, 222
 Wolfsbane, blue, 238
 Wollaston, Dr. 149
 Wood, formation of, 73
 Woodward, Dr. 102

Y.

Yu, observations on the, 378

Z.

Zinc, lamination of, 70

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